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PROBLEM SOLVING*

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I *General Heuristics*

THERE is a purposive tension from which no fully awake animal is free. It consists in a readiness to perceive and to act, or, more generally speaking, to make sense of its own situation, both intellectually and practically. From these routine efforts to retain control of itself and of its surroundings, we can see emerging a process of problem solving, when the effort tends to fall into two stages, a first stage of perplexity, followed by a second stage of doing and perceiving which dispels this perplexity. We may say that the animal has seen a problem if its perplexity lasts for some time and we can clearly recognise that it tries to find a solution to the situation which puzzles it. In doing so the animal is searching for a hidden aspect of the situation, the existence of which it surmises and for the finding or achieving of which the manifest features of the situation serve it as tentative clues or instruments.

To see a problem is a definite addition to knowledge, as much as it is to see a tree or to see a mathematical proof or a joke. It is a surmise which can be true or false, depending on whether the hidden possibilities of which it assumes the existence do actually exist or not. To recognise a problem which can be solved and is worth solving is in fact a discovery in its own right. Famous mathematical problems have descended from generation to generation, leaving in their wake a long trail of achievements stimulated by the attempt at solving them. Accordingly, at the level of animal experiments, we see the psychologist demonstrating to the animal the presence of a problem in order to start it off in search of a solution. A rat in a discrimination box is made to realise that there is food hidden in one of two compartments, both of which are accessible by pushing open its door. Only if he has grasped this will he start searching for a sign which discriminates the door with food behind it from that of the empty compartment.

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Similarly, animals will not start solving a maze unless they are made aware of the fact that there exists a path through it, with some reward at its outlet. In Köhler's 'insight' experiments his chimpanzees grasped their problem from the start and marked their appreciation of the task by composing themselves quietly to concentrate on it.

Accident usually plays some part in discovery and its part may be predominant. Learning experiments can be so arranged that, in the absence of any definitely understood problem, discovery can only be accidental.[1] Mechanistically minded psychologists who devise such experiments would explain all learning as the lucky outcome of random behaviour. This conception of learning underlies also the cyberneticist model of a machine which 'learns' by selecting a 'habit' which has proved successful in a series of random trials. I shall disregard this model of heuristics and continue to explore the process of discovery resulting from intelligent effort irrespective of the neural model that may be proposed for it.

Intelligent problem-solving is manifested among animals most dramatically in Köhler's experiments on chimpanzees, whose behaviour already presents the characteristic stages through which, according to Poincaré, discovery is achieved in mathematics. I have already mentioned the first: the appreciation of a problem. A chimpanzee in a cage within sight of a bunch of bananas out of its reach, neither makes any futile effort to get hold of it by sheer force, nor abandons its desire of acquiring the prize, but settles down instead to an unusual calm, while its eyes survey the situation all round the target; it has recognised the situation as problematical and is searching for a solution.[2] We may acknowledge this (using the terminology of Wallas based on Poincaré) as the stage of Preparation.[3]

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[1] Guthrie and Horton placed a cat in a cage in which a small pole placed in the midst of the floor acted as release mechanism. Cats who had touched the pole by accident and found themselves freed in consequence, quickly realised the connection and proceeded to repeat their releasing action in an exactly stereotype manner. The situation in which the cat was placed offered no intelligible problem to the cat and the solution, found accidentally, showed no clear understanding of the release mechanism; the rôle played by intelligence in the whole process was negligible. (Cf. E. Hilgard, *Theories of Learning*, New York, 1948, p. 68.)

[2] 'The greatest impression on the visitor (writes Köhler) was made when Sultan made a pause, scratching his head leisurely and not moving anything but his eyes and very slightly his head, scrutinising the situation around him in the minutest detail', *The Mentality of Apes*, London, 1927, p. 200.

[3] G. Wallas, *The Art of Thought*, London, pp. 40 sqq.

In the most striking cases of ' insight ' observed by Köhler, this preparatory stage is suddenly followed by intelligent action. Sharply breaking its calm, the animal proceeds to carry out a stratagem by which it secures its aim, or at least shows that it has grasped a principle by which this can be done. Its unhesitating manner suggests that it is guided by a clear conception of its proposed operation. This conception is its discovery, or at least—since it may not always prove practicable—its tentative discovery. We may recognise in its coming the stage of Illumination.

The practical realisation of the principle discovered by insight often presents difficulties, which may even prove unsurmountable. The manipulations by which the animal puts his insight to the test of practical realisation may be regarded as the stage of Verification.

Actually, Poincaré observed four stages of discovery : Preparation, Incubation, Illumination, Verification. But the second of these, Incubation, can be observed only in a rudimentary form in chimpanzees. Yet the observation described in some detail by Köhler, in which one of his animals sustained its effort by solving a problem even while otherwise occupied for a while,[1] anticipates to a remarkable extent the process of Incubation : that curious persistence of heuristic tension through long periods of time during which the problem is not consciously entertained.

An extensive preoccupation with a problem imposes an emotional strain, and a discovery which releases us from it is a great joy. The story of Archimedes rushing out from his bath into the streets of Syracuse, shouting 'Heureka ! ' is a witness to this ; and the account I have quoted from Köhler of the way his chimpanzees behaved before and after solving a problem suggests that they also experience such emotions. I shall show this more definitely later. I mention it now only to make clear that nothing is a problem or discovery in itself; it can be a problem only if it puzzles and worries somebody and a discovery only if it relieves somebody from the burden of a problem. A chess problem means nothing to a chimpanzee or to an imbecile and hence does not puzzle them; a great chess master on the other hand may fail to be puzzled by it because he finds its solution without effort; only a player whose ability is about equal to the problem will find intense preoccupation in it. Only such a player will appreciate its solution as a discovery.

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[1] An ape which for a while had been searching for a tool to rake in a bunch of bananas lying outside its cage, and had made various fruitless attempts in this direction—such as trying to break off a board from the lid of a wooden case or hitting out with a stalk of straw in the direction of the prize—had apparently abandoned the task altogether. It went on playing with one of its fellows for about 10 minutes without turning again to the bananas outside the cage. Then suddenly, its attention having been diverted from its game by a shout nearby, its eyes happened to fall on a stick attached to the roof of the cage and at once it went for the stick and by jumping up a number of times finally secured it and hauled in the bananas by its aid. We may take this to show that even while otherwise occupied the animal kept its problem alive ' at the back of its mind ', keeping it ready to pounce on the instruments of a solution when they happened to meet its eye. Köhler, op. cit., p. 184.

It appears possible to appraise the comparative hardness of a problem and to test the intelligence of subjects by their capacity for solving problems of a certain degree of hardness. The intelligence of chimpanzees and the hardness of certain problems were both successfully assessed by Köhler when he devised a series of problems which some of his apes could solve with some effort while others among them usually failed altogether to do so. The success of Yerkes in setting problems to earthworms (which these could solve after about a hundred trials), shows that he could assess even such extremely low powers of intelligence as were required here from the earthworm.[1] Editors of a crossword column undertake a similar feat in supplying their readers with a steady stream of equally difficult problems. We may conclude that an observer can recognise a problem as such in respect to identifiable persons—even though we admit that there are no problems outside a relation to *some* kind of person.

If an animal who has solved a problem is placed once more in the original situation, it proceeds unhesitatingly to apply the solution which it had originally discovered at the cost of much effort and perhaps many unsuccessful trials. This shows that by solving the problem the animal has acquired a new intellectual power which prevents it from being ever again puzzled by the problem. Instead, it can now deal with the situation in a routine manner involving no heuristic tension and achieving no discovery. The problem has ceased to exist for it.

This irreversible character of heuristic acts is important. It suggests that no solution of a problem can be accredited as a discovery if it is achieved by a procedure following definite rules. For such a procedure would be reversible in the sense that it could be traced back stepwise to its beginning and repeated once more any number of times, like any arithmetical computation. Accordingly, any strictly formalised procedure would also be excluded as a means of achieving discovery.

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[1] R. M. Yerkes, 'The Intelligence of Earthworms', *Jour. Anim. Behav.*, 1912, 2, 332-352; cf. N. R. F. Maier and T. Schneirla, *Principles of Animal Psychology*, New York and London, 1935, pp. 98-101.

It would follow that true discovery is not a strictly logical performance. Accordingly, we may describe the obstacle to be overcome in solving a problem as a ' logical gap ', and speak of the width of the logical gap as the measure of the ingenuity required for solving the problem. ' Illumination ' is then the leap by which the logical gap is crossed. It is the plunge by which we gain a foothold at another shore of reality. On such plunges the scientist has to stake bit by bit his entire professional life.

The width of the logical gap crossed by an inventor is subject to legal assessment. Courts of law are called upon to decide whether the ingenuity displayed in a suggested technical improvement is high enough to warrant its legal recognition as an invention or is merely a routine improvement, achieved by the application of known rules of the art. The invention must be acknowledged to be unpredictable, a quality which is assessed by the intensity of the surprise it might reasonably have aroused. This unexpectedness corresponds precisely to the presence of a logical gap between the antecedent knowledge from which the inventor started and the consequent discovery at which he arrived.

Established rules of inference offer public paths for drawing intelligent conclusions from existing knowledge. The pioneer mind which reaches its own distinctive conclusions by a leap across a logical gap deviates from the commonly accepted process of reasoning, to achieve surprising results. Such an act is original in the sense of making a new start, and the capacity for initiating it is the gift of originality ; a gift possessed by a small minority.

Since the Romantic movement originality has become increasingly recognised as a native endowment which alone enables a person to initiate an essential innovation. Universities and industrial research laboratories are founded today on the employment of persons with original minds. Permanent appointments are given to young scientists who are credited with signs of originality, in the expectation that they will continue to produce surprising ideas for the rest of their lives.

Admittedly, there are minor heuristic acts within the power of ordinary intelligence and indeed continuous with the adaptive capacities of life down to its lowest levels. The interpretative framework of the educated mind is ever ready to meet somewhat novel experiences and to deal with them in a somewhat novel manner. In this sense all life is endowed with originality and originality of a higher order is but a magnified form of a universal biological adaptivity. But genius makes contact with reality on an exceptionally wide range : by seeing problems and reaching out to hidden possibilities for solving them, far beyond the anticipatory powers of current conceptions. Moreover, by deploying such powers in an exceptional measure—far surpassing our own as onlookers—the work of genius offers us a massive demonstration of a creativity which can neither be explained in other terms nor taken unquestioningly for granted. In confrontation with genius we are forced to acknowledge the originative power of life, which we might and commonly do overlook in its ubiquitous lesser manifestations ; for by paying respect to another person's judgment as superior to our own, we emphatically acknowledge originality in the sense of a performance the procedure of which we cannot specify.

In choosing a problem the investigator takes a decision fraught with risks. The task may be insoluble or just too difficult. In that case his effort will be wasted and with it the effort of his collaborators, as well as the money spent on the whole project. But to play safe may be equally wasteful. Mediocre results are no adequate return for the employment of high gifts, and may not even repay the money spent on achieving them. So the choice of a problem must not only anticipate something that is hidden and yet not inaccessible but also assess the investigator's own ability (and those of his collaborators) against the anticipated hardness of the task, and make a reasonable guess as to whether the hoped-for solution will be worth its price in terms of talent, labour, and money. To form such estimates of the approximate feasibility of yet unknown prospective procedures leading to unknown prospective results is the day-to-day responsibility of anyone undertaking independent scientific or technical research. On such grounds as these he must even compare a number of different suggestions and select from them for attack the most promising problem. Yet experience shows that such a performance is possible and can even be relied upon with a considerable degree of probability.

2 *Heuristic Maxims*

There are three major fields of knowledge in which discoveries are possible : natural science, technology, and mathematics. I have referred to examples from each of these fields to illustrate the anticipatory powers which guide discovery. These are clearly quite similar in all three cases. Yet the efforts of philosophers have been almost wholly concentrated on the process of empirical discovery which underlies the natural sciences. Ever since the rise of empiricism at the turn of the sixteenth century philosophers of science have been preoccupied with an attempt to define and justify the process of induction, while by contrast, nobody seems to have tried to define and justify the process by which technical innovations are made, as for example when a new machine is invented. The process of discovery in mathematics has received some attention, and has recently been attacked both from the logical and psychological point of view, but neither approach has raised any epistemological questions parallel to those so sedulously pursued for centuries in connection with empirical induction. It seems to me that any serious attempt to analyse the process of discovery should be sufficiently general to apply to all three fields of systematic knowledge, and so I should like here to identify and acknowledge the powers on which we rely in solving mathematical problems. For reasons of space I shall exclude the history of major discoveries which often involve modifications in the foundations of mathematics and shall attend only to the type of problems that are set to students in teaching them mathematics. Since the solution of these problems is not known to the student the process of finding it bears the marks of a discovery, even though it involves no fundamental change of outlook.

The fact that the teaching of mathematics relies heavily on practice, shows that mathematical knowledge can be acquired only by developing an art; the art of solving mathematical problems. The same is true not only of mathematics and formal logic, but equally also of all mathematical sciences, like mechanics, electrodynamics, thermodynamics, and the mathematical branches of engineering; you cannot master any of these subjects without working out concrete problems in them. The art you strive for in such practical courses is that of converting a language, so far only receptively assimilated, into an effective tool for interpreting a new subject matter, which in this case consists in solving problems.

Thus the process by which mathematics is taught shows once more that the solving of mathematical problems is a heuristic act which leaps across a logical gap. While we cannot expect to find any strict rules for performing such an act, we may expect to discover certain rules of art, the interpretation of which is itself a part of the very art for the pursuit of which they offer us guidance. This is confirmed by the fact that the maxims of problem solving can themselves be learnt only by practice. It is indeed above all the art of heuristic reasoning that the practical teaching of mathematics seeks to impart. This seems to me clearly proven by the comprehensive studies of G. Polya on the subject of mathematical heuristics, on which I shall lean heavily for this study.[1]

The simplest heuristic effort is to search for an object you have mislaid. When I am looking for my fountain pen I know what I expect to find ; I can name it and describe it. Though I know much more about my fountain pen than I can ever recall, and do not know exactly where I left it, the pen is clearly known to me and I know also that it is somewhere within a certain region, though I do not know where. My knowledge of the thing I am looking for is much less ample when I am looking for a word to fit into a crossword puzzle. This time I know only that the missing word has a certain number of letters and designates, for example, something that is badly needed in the Sahara or flows out of a central chimney. These properties are merely clues to a word that I definitely do not know ; clues from which I must try to gain an intimation of what the unknown word may be. Again, a name which I know well but cannot recall at the moment lies somewhere halfway between these two cases. It is more closely present to my mind than the unknown solution of a crossword puzzle, but less closely perhaps than the mislaid fountain pen and its unknown location. Mathematical problems are in the class of crossword puzzles, for to solve such a problem we must find (or construct) something that we have never seen before, with the given data serving us as clues to it.

A problem may admit of a systematic solution. By ransacking my flat inch by inch I may make sure of eventually finding my fountain pen, which I know to be somewhere in it. I might solve a chess problem by trying out mechanically all combination of possible moves and countermoves. Systematic methods apply also to many mathematical problems. though usually they are far too laborious to be carried out in practice.[2] It is clear that any such systematic operation would reach a solution without crossing a logical gap and would not constitute a heuristic act.

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[1] G. Polya, *How to Solve it*, Princeton, 1945, and *Mathematics and Plausible Reasoning*, Vols. 1 and 2, London, 1954. Penetrating observations on problem solving have also been contributed by psychologists, mainly Duncker and Wertheimer.

[2] A. M. Turing (*Science News*, 1954, 31) has computed the number of arrangements that would have to be surveyed in the process of solving systematically a very common form of puzzle consisting of sliding squares to be rearranged in a particular way. The number is 20,922,789,888,000. Working continuously day and night and inspecting one position per minute the process would take 4 million years.

The difference between the two kinds of problem solving, the systematic and the heuristic, reappears in the fact that while a systematic operation is a wholly deliberate act, a heuristic process is a combination of active and passive stages. A deliberate heuristic activity is performed during the stage of Preparation. If this is followed by a period of Incubation, nothing is done and nothing happens on the level of consciousness for this time. The advent of a bright idea (whether following immediately from Preparation or only after an interval of Incubation) is the fruit of the investigator's earlier efforts, but not itself an action on his part; it just happens to him. And again, the testing of the 'bright idea' by a formal process of Verification, is another deliberate action of the investigator. However, the decisive act of discovery must have occurred before this, at the moment when the happy thought emerged.

Though the solution of a problem is something we have never met before, yet in the heuristic process it plays a part similar to the mislaid fountain pen or the forgotten name which we know quite well. We are looking for it as if it were there, pre-existent. Problems set to students are of course known to have a solution; but the belief that there exists a hidden solution which we may be able to find, is essential also in envisaging and working at a yet unsolved problem. It determines also the manner in which the 'happy thought' eventually presents itself as something inherently satisfying. It is not one among a great many ideas to be pondered upon at leisure, but one which carries conviction from the start. We shall see in a moment that this is a necessary consequence of the way a heuristic striving evokes its own consummation. To the closer analysis of this process I shall now turn.

A problem is an intellectual desire (a 'quasi-need' in K. Lewin's terminology) and like every desire it postulates the existence of something that can satisfy it; in the case of a problem its satisfier is its solution. As all desire stimulates the imagination to dwell on the means of satisfying it, and is stirred up in its turn by the play of the imagination it has fostered, so also by taking interest in a problem we start speculating about its possible solution and in doing so become further engrossed in the problem.

Obsession with one's problem is in fact the mainspring of all inventive power. Asked by his pupils in jest, what they should do to become 'a Pavlov', the master answered in all seriousness: 'Get up in the morning with your problem before you. Breakfast with it. Go to the laboratory with it. Eat your lunch with it. Keep it before you after dinner. Go to bed with it in your mind. Dream about it.'[1] It is the unremitting preoccupation with a problem that lends to genius its proverbial capacity for taking infinite pains. And the intensity of our preoccupation with a problem generates also our power for re-organising our thoughts successfully, both during the hours of search and afterwards, during a period of rest.[2]

But what is the object of this intensive preoccupation? Can we concentrate our attention on something we don't know? Yet this is precisely what we are told to do: 'Look at the unknown!' says Polya, 'Look at the end. Remember your aim. Do not lose sight of what is required. Keep in mind what you are working for. *Look at the unknown. Look at the conclusion.*'[3] No advice could be more emphatic.

The seeming paradox is resolved by the fact that even though we have never met the solution we have a conception of it in the same sense as we have a conception of a forgotten name. By directing our attention on a focus in which we are subsidiarily aware of all the particulars that remind us of the forgotten name, we form a conception of it; and likewise, by fixing our attention on a focus in which we are subsidiarily aware of the data by which the solution of a problem is determined, we form a conception of this solution. The admonition to look at the unknown really means that we should *look at the known data, not, however, in themselves, but as clues to the unknown; as pointers to it and parts of it.* We should make every effort to feel our way to an understanding of the manner in which these known particulars hang together both mutually and with the unknown. Thus we make sure that the unknown is really there, essentially determined by what is known about it, and able to satisfy all the demands made on it by the problem.

All our conceptions have heuristic powers; they are ever ready to identify novel instances of experience by modifying themselves so as to comprise them. The practice of skills likewise is inventive; by concentrating our purpose on the achievement of success we evoke ever new capacities in ourselves. A problem partakes of both these types of endeavour. It is a conception of something we are striving for. It is an intellectual desire for crossing a logical gap on the other side of which lies the unknown, fully marked out by our conception of it, though as yet never seen in itself. The search for a solution consists in casting about with this purpose in mind. This we do by performing two operations which must always be tried jointly. We must (i) set out the problem in suitable symbols and continuously reorganise its representation with a view to eliciting some new suggestive aspects of it and concurrently (ii) ransack our memory for any similar problem of which the solution is known. The scope of these two operations will usually be limited by the student's technical facility for transforming the given data in different ways and by the range of germane theorems with which he is acquainted. But his success will depend ultimately on his capacity for sensing the presence of yet unrevealed logical relations between the conditions of the problem, the theorems known to him, and the unknown solution he is looking for. Unless his casting about is guided by a reliable sense of growing proximity to the solution, he will make no progress towards it. Conjectures made at random, even though following the best rules of heuristics, would be hopelessly inept and totally fruitless.

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[1] J. R. Baker, *Science and the Planned State*, London, 1945, p. 55

[2] 'Only such problems come back improved after a rest whose solution we passionately desire and for which we have worked with great tension' writes Polya (op. cit. p. 172).

[3] Ibid., p. 112

The process of solving a mathematical problem continues to depend therefore at every stage on the same ability to anticipate a hidden potentiality which enables the student to see a problem in the first place and set out to solve it. Polya has compared a mathematical discovery consisting of a whole chain of consecutive steps with an arch where every stone depends for its stability on the presence of others, and pointed out the paradox that the stones are in fact put in one at a time. Again, the paradox is resolved by the fact that each successive step of the incomplete solution is upheld by the heuristic anticipation which originally evoked its invention: by the feeling that its emergence has narrowed further the logical gap of the problem.

The growing sense of approaching to the solution of a problem can be commonly experienced when we grope for a forgotten name. We all know the exciting sense of increasing proximity to the missing word which we may confidently express by saying ; 'I shall remember it in a moment' and perhaps later 'It is on the tip of my tongue'. The expectation expressed by such words is often confirmed in the event. I believe that we should likewise acknowledge our capacity both to sense the accessibility of a hidden inference from given premisses and to invent transformations of the premisses which increase the accessibility of the hidden inference. We should recognise that this foreknowledge biases our guesses in the right direction, so that their probability of hitting the mark, which would otherwise be zero, becomes so high that we can definitely rely on it simply on the grounds of a student's intelligence : or for higher performances, on the grounds of the special gifts possessed by the professional mathematician.

The feeling that the logical gap separating us from the solution of a problem has been reduced means that less work should remain to be done for solving it. It may also mean that the rest of the solution will be comparatively easy or that it may present itself without further effort on our part, after a period of rest. The fact that our intellectual strivings make effective progress during a period of Incubation without any effort on our part is in line with the latent character of all knowledge. As we continuously know a great many things without always thinking of them, so we naturally also keep on desiring or fearing all manner of things without always thinking of them. We know how a set purpose may result in action automatically later, as when we go to bed resolved to wake up at a certain hour. Post-hypnotic suggestions can set going latent processes which compulsively result after a number of hours in the performance requested of the subject.[1] Mrs Zeigarnik has shown that unfinished tasks continue likewise to preoccupy us unconsciously ; their memory persists after finished tasks are forgotten.[2] The fact that the tension set up by the unfinished task continues to make progress towards its fulfilment, is shown by the well-known experience of sportsmen that a period of rest following on a spell of intensive training produces an improvement of skill. The spontaneous success of the search for a forgotten name or for the solution of a problem, after a period of quiescence, falls in line with this experience.

These antecedents explain also the manner in which the final success of problem solving will suddenly set in. For each step, whether spontaneous or contrived, that brings us nearer to the solution increases our premonition of its proximity and brings a more concentrated effort to bear on a reduced logical gap. The last stage of the solution may therefore be frequently achieved in a self-accelerating manner and the final discovery may be upon us in a flash.

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[1] Cf. N. Ach, 'Determining Tendencies ; Awareness', in D. Rappaport, *Organisation and Pathology of Thought*, New York, 1951, pp. 16 sqq.

[2] W. D. Ellis, *A Source Book of Gestalt Psychology*, New York, 1938, pp. 300–314

I have said that our heuristic cravings imply, like our bodily appetites, the existence of something which has the properties required to satisfy us, and that the intimations which guide our striving express this belief. But the satisfier of our craving has in this case no bodily existence ; it is not a hidden object but an idea never yet conceived.

We hope that as we work at the problem this idea will come to us, whether all at once or bit by bit ; and only if we believe that this solution exists can we passionately search for it and evoke from ourselves heuristic steps towards its discovery. Therefore as it emerges in response to our search for something we believe to be there, discovery, or supposed discovery, will always come to us with the conviction of its being true. It arrives accredited in advance by the heuristic craving which evoked it.

The most daring feats of originality are still subject to this law : they must be performed in the assumption that they originate nothing, but merely reveal what is there. And their triumph confirms this assumption, for what has been found bears the mark of reality in being pregnant with yet unforeseeable implications. Mathematical heuristics, aiming at conceptual reorganisation without reference to new experience, once more exemplifies in its own terms that an intellectual striving entails the conviction of anticipating reality. It illustrates also how this conviction finds itself confirmed by the eventual solution, which ' solves ' precisely because it successfully claims to reveal an aspect of reality. And we can see again, finally, that this whole process of discovery and confirmation ultimately relies on our own accrediting of our own vision of reality.

To start working on a mathematical problem, we reach for pencil and paper, and throughout the stage of Preparation we keep trying out ideas on paper in terms of symbolic operations. If this does not lead straight to success, we may have to think the whole matter over again, and may perhaps see the solution revealed much later unexpectedly in a moment of Illumination. Actually, however, such a flash of triumph usually offers no solution, but only envisages a solution, which has yet to be tested. In the verification or working out of the solution we must again rely therefore on explicit symbolic operations. Thus both the first active steps undertaken to solve a problem and the final garnering of the solution rely effectively on computations and other symbolic operations, while the more informal act by which the logical gap is crossed lies between these two formal procedures. However, the intuitive powers of the investigator are always dominant and decisive. Good mathematicians are usually found capable of carrying out computations quickly and reliably, for unless they command this technique they may fail to make their ingenuity effective ; but their ingenuity itself lies in producing ideas. Hadamard says that he used to make more mistakes in calculations than his own pupils but that he more quickly discovered them because the result did not *look* right ; it is almost as if by his computations he had been merely drawing a portrait of his conceptually prefigured conclusions.[1] Gauss is widely quoted as having said : ' I have had my solutions for a long time but I do not yet know how I am to arrive at them.' Though the quotation may be doubtful it remains well said.[2] A situation of this kind certainly prevails every time we discover what we believe to be the solution to a problem. At this moment we have the vision of a solution which *looks* right and which we are therefore confident will *prove* right.[3]

The manner in which the mathematician works his way towards discovery by shifting his confidence from intuition to computation and back again from computation to intuition, while never releasing his hold on either of the two, represents in miniature the whole range of operations by which articulation disciplines and expands the reasoning powers of man. This alternation is asymmetrical, for a formal step can be valid only by virtue of our tacit confirmation of it. Moreover, a symbolic formalism is itself but an embodiment of our antecedent unformalised powers ; it is an instrument skilfully contrived by our inarticulate selves for the purpose of relying on it as our external guide. The interpretation of primitive terms and axioms is predominantly inarticulate and so is the process of their expansion and re-interpretation, which underlies the progress of mathematics. A formal proof proves nothing until it induces the tacit conviction that it is binding. Thus the alternation between the intuitive and the formal depends on tacit affirmations both at the beginning and at the end of each chain of formal reasoning.

This brings us back to the purposive tension that we share with animals, as the primordial root of problem-solving at all levels. But the rooting of our highest articulate powers in the inarticulate has far wider implications. If everywhere it is the inarticulate that has the last word, unspoken and yet decisive, then a corresponding reform of the whole conception not only of problem-solving but of truth in general, is itself inevitable. The ideal of an impersonally detached truth must be abridged to allow for the inherently personal character of the act by which truth is declared. It is the endeavour to effect such a revision of which the preceding inquiry into problem-solving forms part.

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[1] J. Hadamard, *The Psychology of Invention in the Mathematical Field*, Princeton, 1945, p. 49

[2] Agnes Arber, *The Mind and the Eye*, Cambridge, 1954, p. 47

[3] Archimedes describes in his *Method* a mechanical process of geometrical demonstration which carries conviction with him, though he regards its results as still requiring proof, which he proceeds to supply. B. L. Van der Waerden, *Science Awakening*, Groningen, 1954, p. 215.

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HISTORICAL EXPLANATION IN THE SOCIAL SCIENCES *

J. W. N. Watkins

1 *Introduction*

The hope which originally inspired methodology was the hope of finding a method of enquiry which would be both necessary and sufficient to guide the scientist unerringly to truth. This hope has died a natural death. Today, methodology has the more modest task of establishing certain rules and requirements which are necessary to prohibit some wrong-headed moves but insufficient to guarantee success. These rules and requirements, which circumscribe scientific enquiries without steering them in any specific direction, are of the two main kinds, formal and material. So far as I can see, the formal rules of scientific method (which comprise both logical rules and certain realistic and fruitful stipulations) are equally applicable to all the empirical sciences. You cannot, for example, deduce a universal law from a finite number of observations whether you are a physicist, a biologist, or an anthropologist. Again, a single comprehensive explanation of a whole range of phenomena is preferable to isolated explanations of each of those phenomena, whatever your field of enquiry. I shall therefore confine myself to the more disputable (I had nearly said ' more disreputable ') and metaphysically impregnated part of methodology which tries to establish the appropriate *material* requirements which the *contents* of the premisses of an explanatory theory in a particular field ought to satisfy. These requirements may be called regulative principles. Fundamental differences in the subject-matters of different sciences—differences to which formal methodological rules are impervious—ought, presumably, to be reflected in the regulative principles appropriate to each science. It is here that the student of the methods of the social sciences may be expected to have something distinctive to say.

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* A revised version of a paper read at the First Annual Conference of the Philosophy of Science Group, Manchester, on 23rd September 1956. Footnotes have been added subsequently.

An example of a regulative principle is mechanism, a metaphysical theory which governed thinking in the physical sciences from the seventeenth century until it was largely superseded by a wave or field world-view. According to mechanism, the ultimate constituents of the physical world are impenetrable particles which obey simple mechanical laws. The existence of these particles cannot be explained—at any rate by science. On the other hand, every complex physical thing or event is the result of a particular configuration of particles and can be explained in terms of the laws governing their behaviour in conjunction with a description of their relative positions, masses, momenta, etc. There may be what might be described as unfinished or half-way explanations of large-scale phenomena (say, the pressure inside a gas-container) in terms of other large-scale factors (the volume and temperature of the gas); but we shall not have arrived at rock-bottom explanations of such large-scale phenomena until we have deduced their behaviour from statements about the properties and relations of particles.

This is a typically metaphysical idea (by which I intend nothing derogatory). True, it is confirmed, even massively confirmed, by the huge success of mechanical theories which conform to its requirements. On the other hand, it is untestable. No experiment could overthrow it. If certain phenomena—say, electromagnetic phenomena—seem refractory to this mechanistic sort of explanation, this refractoriness can always (and perhaps rightly) be attributed to our inability to find a successful mechanical model rather than to an error in our metaphysical intuition about the ultimate constitution of the physical world. But while mechanism is weak enough to be compatible with any *observation* whatever, while it is an untestable and unempirical principle, it is strong enough to be incompatible with various conceivable physical *theories*. It is this which makes it a *regulative*, non-vacuous metaphysical principle. If it were compatible with everything it would regulate nothing. Some people complain that regulative principles discourage research in certain directions, but that is a part of their purpose. You cannot encourage research in one direction without discouraging research in rival directions.

I am not an advocate of mechanism but I have mentioned it because I am an advocate of an analogous principle in social science, the principle of methodological individualism.[1] According to this principle, the ultimate constituents of the social world are individual people who act more or less appropriately in the light of their dispositions and understanding of their situation. Every complex social situation, institution, or event is the result of a particular configuration of individuals, their dispositions, situations, beliefs, and physical resources and environment. There may be unfinished or half-way explanations of large-scale social phenomena (say, inflation) in terms of other large-scale phenomena (say, full employment); but we shall not have arived at rock-bottom explanations of such large-scale phenomena until we have deduced an account of them from statements about the dispositions, beliefs, resources, and inter-relations of individuals. (The individuals may remain anonymous and only typical dispositions, etc., may be attributed to them.) And just as mechanism is contrasted with the organicist idea of physical fields, so methodological individualism is contrasted with sociological holism or organicism. On this latter view, social systems constitute 'wholes' at least in the sense that some of their large-scale behaviour is governed by macro-laws which are essentially *sociological* in the sense that they are *sui generis* and not to be explained as mere regularities or tendencies resulting from the behaviour of interacting individuals. On the contrary, the behaviour of individuals should (according to sociological holism) be explained at least partly in terms of such laws (perhaps in conjunction with an account, first of individuals' rôles within institutions and secondly of the functions of institutions within the whole social system). If methodological individualism means that human beings are supposed to be the only moving agents in history, and if sociological holism means that some superhuman agents or factors are supposed to be at work in history, then these two alternatives are exhaustive. An example of such a superhuman, sociological factor is the alleged long-term cyclical wave in economic life which is supposed to be self-propelling, uncontrollable, and inexplicable in terms of human activity, but in terms of the fluctuations of which such large-scale phenomena as wars, revolutions, and mass emigration, and such

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[1] Both of these analogous principles go back at least to Epicurus. In recent times methodological individualism has been powerfully defended by Professor

F. A. Hayek in his *Individualism and Economic Order* and *The Counter-Revolution of Science*, and by Professor K. R. Popper in his *The Open Society and its Enemies* and 'The Poverty of Historicism' *Economica*, 1944-45, **11-12**. Following in their footsteps I have also attempted to defend methodological individualism in 'Ideal Types and Historical Explanation' this *Journal*, 1952, **3**, 22, reprinted in *Readings in the Philosophy of Science*, ed. Feigl and Brodbeck, New York, 1953. This article has come in for a good deal of criticism, the chief items of which I shall try to rebut in what follows.

psychological factors as scientific and technological inventiveness can, it is claimed, be explained and predicted.

I say ' and predicted ' because the irreducible sociological laws postulated by holists are usually regarded by them as laws of social development, as laws governing the dynamics of a society. This makes holism well-nigh equivalent to historicism, to the idea that a society is impelled along a pre-determined route by historical laws which cannot be resisted but which can be discerned by the sociologist. The holist-historicist position has, in my view, been irretrievably damaged by Popper's attacks on it. I shall criticise this position only in so far as this will help me to elucidate and defend the individualistic alternative to it. The central assumption of the individualistic position —an assumption which is admittedly counter-factual and metaphysical—is that no social tendency exists which could not be altered *if* the individuals concerned both wanted to alter it and possessed the appropriate information. (They might want to alter the tendency but, through ignorance of the facts and/or failure to work out some of the implications of their action, fail to alter it, or perhaps even intensify it.) This assumption could also be expressed by saying that no social tendency is somehow imposed on human beings ' from above ' (or ' from below ')—social tendencies are the product (usually undesigned) of human characteristics and activities and situations, of people's ignorance and laziness as well as of their knowledge and ambition. (An example of a social tendency is the tendency of industrial units to grow larger. I do not call ' social ' those tendencies which are determined by uncontrollable physical factors, such as the alleged tendency for more male babies to be born in times of disease or war.)[1]

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[1] The issue of holism *versus* individualism in social science has recently been presented as though it were a question of the existence or non-existence of irreducibly social *facts* rather than of irreducibly sociological *laws*. (See M. Mandelbaum, ' Societal Facts ', *The British Journal of Sociology*, 1955, **6**, and E. A. Gellner, ' Explanations in History ', *Aristotelian Society*, Supplementary Volume **30**, 1956.) This way of presenting the issue seems to me to empty it of most of its interest. If a new kind of beast is discovered, what we want to know is not so much whether it falls outside existing zoological categories, but how it behaves. People who insist on the existence of social facts but who do not say whether they are governed by sociological laws, are like people who claim to have discovered an unclassified kind of animal but who do not tell us whether it is tame or dangerous, whether it can be domesticated or is unmanageable. If an answer to the question of social facts could throw light on the serious and interesting question of sociological laws, then the question of social facts would also be serious and interesting. But this is not so. On the one hand, a holist may readily admit (as I pointed out in my ' Ideal

My procedure will be: first, to de-limit the sphere in which methodological indivisualism works in two directions; secondly, to clear methodological individualism of certain misunderstandings; thirdly, to indicate how fruitful and surprising individualistic explanations can be and how individualistic social theories can lead to sociological discoveries; and fourthly, to consider in somewhat more detail how, according to methodological individualism, we should frame explanations, first for social regularities or repeatable processes, and secondly for unique historical constellations of events.

2 *Where Methodological Individualism Does not Work*

There are two areas in which methodological individualism does not work.

The first is a probability situation where accidental and unpredictable irregularities in human behaviour have a fairly regular and predictable overall result.[1] Suppose I successively place 1,000 individuals facing north in the centre of a symmetrical room with two exits, one east, the other west. If about 500 leave by one exit and about 500 by the other I would not try to explain this in terms of tiny undetectable west-inclining and east-inclining differences in the individuals, for the same reason that Popper would not try to explain

Types' paper, which Gellner criticises) that all observable social facts *are* reducible to individual facts and yet hold that the latter are invisibly governed by irreducibly sociological laws. On the other hand, an individualist may readily admit (as Gellner himself says) that some large social facts are simply too complex for a full reduction of them to be feasible, and yet hold that individualistic explanations of them are in principle possible, just as a physicist may readily admit that some physical facts (for instance, the precise blast-effects of a bomb-explosion in a built-up area) are just too complex for accurate prediction or explanation of them to be feasible and yet hold that precise explanations and predictions of them in terms of existing scientific laws are in principle possible.

This revised way of presenting the holism *versus* individualism issue does not only divert attention from the important question. It also tends to turn the dispute into a purely verbal issue. Thus Mandelbaum is able to prove the existence of what he calls 'societal facts' because he defines psychological facts very narrowly as 'facts concerning the thoughts and actions of specific human beings' (op. cit. p. 307). Consequently, the *dispositions* of *anonymous* individuals which play such an important rôle in individualistic explanations in social science are 'societal facts' merely by definition.

[1] Failure to exclude probability-situations from the ambit of methodological individualism was an important defect of my 'Ideal Types' paper. Here, Gellner's criticism (op. cit. p. 163) does hit the nail on the head.

the fact that about 500 balls will topple over to the west and about 500 to the east, if 1,000 balls are dropped from immediately above a north-south blade, in terms of tiny undetectable west-inclining and east-inclining differences in the balls. For in both cases such an ' explanation ' would merely raise the further problem : why should these west-inclining and east-inclining differences be distributed approximately *equally* among the individuals and among the balls ?

Those statistical regularities in social life which are inexplicable in individualistic terms for the sort of reason I have quoted here are, in a sense, inhuman, the outcome of a large number of sheer *accidents*. The outcome of a large number of decisions is usually much less regular and predictable because variable human factors (changes of taste, new ideas, swings from optimism to pessimism) which have little or no influence on accident-rates are influential here. Thus Stock Exchange prices fluctuate widely from year to year, whereas the number of road-accidents does not fluctuate widely. But the existence of these actuarial regularities does not, as has often been alleged, support the historicist idea that defenceless individuals like you and me are at the chance mercy of the inhuman and uncontrollable tendencies of our society. It does not support a secularised version of the Calvinist idea of an Almighty Providence who picks people at random to fill His fixed damnation-quota. For we can control these statistical regularities in so far as we can alter the conditions on which they depend. For example, we could obviously abolish road-accidents if we were prepared to prohibit motor-traffic.

The second kind of social phenomenon to which methodological individualism is inapplicable is where some kind of physical connection between people's nervous systems short-circuits their intelligent control and causes automatic, and perhaps in some sense appropriate, bodily responses. I think that a man may more or less literally smell danger and instinctively back away from unseen ambushers ; and individuality seems to be temporarily submerged beneath a collective physical *rapport* at jive-sessions and revivalist meetings and among panicking crowds. But I do not think that these spasmodic mob-organisms lend much support to holism or constitute a very serious exception to methodological individualism. They have a fleeting existence which ends when their members put on their mufflers and catch the bus or otherwise disperse, whereas holists have conceived of a social whole as something which endures through generations of men ; and whatever holds together typical long-lived institutions, like a bank or a legal system or a church, it certainly is not the physical proximity of their members.

3 *Misunderstandings of Methodological Individualism*

I will now clear methodological individualism of two rather widespread misunderstandings.

It has been objected that in making individual dispositions and beliefs and situations the terminus of an explanation in social science, methodological individualism implies that a person's psychological make-up is, so to speak, God-given, whereas it is in fact conditioned by, and ought to be explained in terms of, his social inheritance and environment.[1] Now methodological individualism certainly does not prohibit attempts to explain the formation of psychological characteristics; it only requires that such explanations should in turn be *individualistic*, explaining the formation as the result of a series of conscious or unconscious responses by an individual to his changing situation. For example, I have heard Professor Paul Sweezey, the Harvard economist, explain that he became a Marxist because his father, a Wall Street broker, sent him in the 1930's to the London School of Economics to study under those staunch liberal economists, Professors Hayek and Robbins. This explanation is perfectly compatible with methodological individualism (though hardly compatible, I should have thought, with the Marxist idea that ideologies reflect class-positions) because it interprets his ideological development as a human response to his situation. It is, I suppose, psycho-analysts who have most systematically worked the idea of a thorough individualist and historical explanation of the formation of dispositions, unconscious fears and beliefs, and subsequent defence-mechanisms, in terms of responses to emotionally charged, and especially childhood, situations.

My point could be put by saying that methodological individualism encourages *innocent* explanations but forbids *sinister* explanations of the widespread existence of a disposition among the members of a social group. Let me illustrate this by quoting from a reply I made to Goldstein's criticisms.

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[1] Thus Gellner writes: 'The real oddity of the reductionist [i.e. the methodological individualist's] case is that it seems to preclude *a priori* the possibility of human dispositions being the dependent variable in an historical explanation—when in fact they often or always are' (op. cit. p. 165). And Mr Leon J. Goldstein says that in making human dispositions methodologically primary I ignore their cultural conditioning. (*The Journal of Philosophy*, 1956, **53**, 807.)

> Suppose that it is established that Huguenot traders were relatively prosperous in 17th-century France and that this is explained in terms of a wide-spread disposition among them (a disposition for which there is independent evidence) to plough back into their businesses a larger proportion of their profits than was customary among their Catholic competitors. Now this explanatory disposition might very well be explained in its turn—perhaps in terms of the general thriftiness which Calvinism is said to encourage, and/or in terms of the fewer alternative outlets for the cash resources of people whose religious disabilities prevented them from buying landed estates or political offices. (I cannot vouch for the historical accuracy of this example.)
>
> I agree that methodological individualism allows the formation, or ' cultural conditioning ', of a widespread disposition to be explained only in terms of other human factors and not in terms of something *in*human, such as an alleged historicist law which impels people willy-nilly along some pre-determined course. But this is just the anti-historicist point of methodological individualism.

Unfortunately, it is typically a part of the programme of Marxist and other historicist sociologies to try to account for the formation of ideologies and other psychological characteristics in strictly sociological and non-psychological terms. Marx, for instance, professed to believe that feudal ideas and *bourgeois* ideas are more or less literally generated by the water-mill and the steam-engine. But no description, however complete, of the productive apparatus of a society, or of any other non-psychological factors, will enable you to deduce a single psychological conclusion from it, because psychological statements logically cannot be deduced from wholly non-psychological statements. Thus whereas the mechanistic idea that explanations in physics cannot go behind the impenetrable particles is a prejudice (though a very understandable prejudice), the analogous idea that an explanation which begins by imputing some social phenomenon to human factors cannot go on to explain those factors in terms of some inhuman determinant of them is a necessary truth. That the human mind develops under various influences the methodological individualist does not, of course, deny. He only insists that such development must be explained ' innocently ' as a series of responses by the individual to situations and not ' sinisterly ' and illogically as a direct causal outcome of non-psychological factors, whether these are neurological factors, or impersonal sociological factors alleged to be at work in history.

Another cause of complaint against methodological individualism is that it has been confused with a narrow species of itself (Popper calls it ' psychologism ') and even, on occasion, with a still narrower sub-species of this (Popper calls it the ' Conspiracy Theory of Society ').[1] Psychologism says that all large-scale social characteristics are not merely the intended or unintended result of, but a *reflection* of, individual characteristics.[2] Thus Plato said that the character and make-up of a *polis* is a reflection of the character and make-up of the kind of soul predominant in it. The conspiracy theory says that all large-scale social phenomena (do not merely reflect individual characteristics but) are deliberately brought about by individuals or groups of individuals.

Now there are social phenomena, like mass unemployment, which it would not have been in anyone's interest deliberately to bring about and which do not appear to be large-scale social reflections or magnified duplicates of some individual characteristic. The practical or technological or therapeutic importance of social science largely consists in explaining, and thereby perhaps rendering politically manageable, the unintended and unfortunate consequences of the behaviour of interacting individuals. From this pragmatic point of view, psychologism and the conspiracy theory are unrewarding doctrines. Psychologism says that only a change of heart can put a stop to, for example, war (I think that this is Bertrand Russell's view). The conspiracy theory, faced with a big bad social event, leads to a hunt for scape-goats. But methodological individualism, by imputing unwanted social phenomena to individuals' responses to their situations, in the light of their dispositions and beliefs, suggests that we may be able to make the phenomena disappear, not by recruiting good men to fill the posts hitherto occupied by bad men, nor by trying to destroy men's socially unfortunate dispositions while fostering their socially beneficial dispositions, but simply by altering the situations they confront. To give a current example, by confronting individuals with dearer money and reduced credit the Government may (I do not say will) succeed in halting inflation without requiring a new self-denying attitude on the part of consumers and without sending anyone to prison.

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[1] See K. R. Popper, *The Open Society and its Enemies*, 2nd edn., 1952, ch. 14

[2] I am at a loss to understand how Gellner came to make the following strange assertion : ' . . . Popper refers to both " psychologism " which he condemns, and " methodological individualism ", which he commends. When in the articles discussed [i.e., my " Ideal Types " paper] " methodological individualism" is worked out more fully than is the case in Popper's book, it seems to me to be indistinguishable from " Psychologism ".' Finding no difference between methodological individualism and a caricature of methodological individualism, Gellner has no difficulty in poking fun at the whole idea : ' Certain tribes I know have what anthropologists call a segmentary patrilineal structure, which moreover maintains itself very well over time. I could " explain " this by saying that the tribesmen have, all or most of them, dispositions whose effect is to maintain the system. But, of course, not only have they never given the matter much thought, but it also might very well be impossible to isolate anything in the characters and conduct of the individual tribesmen which *explains* how they come to maintain the system ' (op. cit. p. 176). Yet this example actually suggests the lines along which an individualistic explanation might be found. The very fact that the tribesmen *have never given the matter much thought*, the fact that they accept their inherited system uncritically, may constitute an important part of an explanation of its stability. The explanation might go on to pin-point certain rules—that is firm and widespread dispositions—about marriage, inheritance, etc., which help to regularise the tribesmen's behaviour towards their kinsmen. How they come to share these common dispositions could also be explained individualistically in the same sort of way that I can explain why my young children are already developing a typically English attitude towards policemen.

4 *Factual Discoveries in Social Science*

To explain the unintended but *beneficial* consequences of individual activities—by ' beneficial consequences ' I mean social consequences which the individuals affected *would* endorse *if* they were called on to choose between their continuation or discontinuation—is usually a task of less practical urgency than the explanation of undesirable consequences. On the other hand, this task may be of greater theoretical interest. I say this because people who are painfully aware of the existence of unwanted social phenomena may be oblivious of the unintended but beneficial consequences of men's actions, rather as a man may be oblivious of the good health to which the smooth functioning of his digestion, nervous system, circulation, etc., give rise. Here, an explanatory social theory may surprise and enlighten us not only with regard to the connections between causes and effect but with regard to the existence of the effect itself. By showing that a certain economic system contains positive feed-back leading to increasingly violent oscillations and crises an economist may explain a range of well-advertised phenomena which have long been the subject of strenuous political agitation. But the economists who first showed that a certain kind of economic system contains negative feed-back which tends to iron out disturbances and restore equilibrium, not only explained, but also revealed the existence of, phenomena which had hardly been remarked upon before.[1]

I will speak of organic-like social behaviour where members of some social system (that is, a collection of people whose activities disturb and influence each other) mutually adjust themselves to the situations created by the others in a way which, without direction from above, conduces to the equilibrium or preservation or development of the system. (These are again evaluative notions, but they can also be given a 'would-be-endorsed-if' definition.) Now such far-flung organic-like behaviour, involving people widely separated in space and largely ignorant of each other, cannot be simply observed. It can only be theoretically reconstructed—by deducing the distant social consequences of the typical responses of a large number of interacting people to certain repetitive situations. This explains why individualistic-minded economists and anthropologists, who deny that societies really are organisms, have succeeded in piecing together a good deal of unsuspected organic-like social behaviour, from an examination of individual dispositions and situations, whereas sociological holists, who insist that societies really are organisms, have been noticeably unsuccessful in convincingly displaying any organic-like social behaviour—they cannot observe it and they do not try to reconstruct it individualistically.

There is a parallel between holism and psychologism which explains their common failure to make surprising discoveries. A large-scale social characteristic should be explained, according to psychologism, as the manifestation of analogous small-scale psychological tendencies in individuals, and according to holism as the manifestation of a large-scale tendency in the social whole. In both cases, the *explicans* does little more than duplicate the *explicandum*. The methodological individualist, on the other hand, will try to explain the large-scale effect as the *indirect,* unexpected, complex product of individual factors none of which, singly, may bear any resemblance to it at all. To use hackneyed examples, he may show that a longing for peace led, in a certain international situation, to war, or that a government's desire to improve a bad economic situation by balancing its budget only worsened the situation. Since Mandeville's *Fable of the Bees* was published in 1714, individualistic social science, with its emphasis on unintended consequences, has largely been a sophisticated elaboration on the simple theme that, in certain situations, selfish private motives may have good social consequences and good political intentions bad social consequences.[1]

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[1] This sentence, as I have since learnt from Dr A. W. Phillips, is unduly complacent, for it is very doubtful whether an economist can ever *show* that an economic system containing negative feed-back will be stable. For negative feed-back may produce either a tendency towards equilibrium, or increasing oscillations, according to the numerical values of the parameters of the system. But numerical values are just what economic measurements, which are usually ordinal rather than cardinal, seldom yield. The belief that a system which contains negative feed-back, but whose variables cannot be described quantitatively, is stable may be based on faith or experience, but it cannot be shown mathematically. See A. W. Phillips, 'Stabilisation Policy and the Time-Forms of Lagged Responses', *The Economic Journal*, 1957, 67.

Holists draw comfort from the example of biology, but I think that the parallel is really between the biologist and the methodological individualist. The biologist does not, I take it, explain the large changes which occur during, say, pregnancy, in terms of corresponding large teleological tendencies in the organism, but physically, in terms of small chemical, cellular, neurological, etc., changes, none of which bears any resemblance to their joint and seemingly planful outcome.

5 *How Social Explanations Should be Framed*

I will now consider how regularities in social life, such as the trade cycle, should be explained according to methodological individualism. The explanation should be in terms of individuals and their situations; and since the process to be explained is repeatable, liable to recur at various times and in various parts of the world, it follows that only very general assumptions about human dispositions can be employed in its explanation. It is no use looking to abnormal psychology for an explanation of the structure of interest-rates—everyday experience must contain the raw material for the dispositional (as opposed to the situational) assumptions required by such an explanation. It may require a stroke of genius to detect, isolate, and formulate precisely the dispositional premisses of an explanation of a social regularity. These premisses may state what no one had noticed before, or give a sharp articulation to what had hitherto been loosely described. But once stated they will seem obvious enough. It took years of groping by brilliant minds before a precise formulation was found for the principle of diminishing marginal utility. But once stated, the principle—that the less, relatively, a man has of one divisible commodity the more compensation he will be disposed to require for foregoing a small fixed amount of it—is a principle to which pretty well everyone will give his consent. Yet this simple and almost platitudinous principle is the magic key to the economics of distribution and exchange.

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[1] A good deal of unmerited opposition to methodological individualism seems to spring from the recognition of the undoubted fact that individuals often run into social obstacles. Thus the conclusion at which Mandelbaum arrives is 'that there are societal facts which exercise external constraints over individuals' (op. cit. p. 317). This conclusion is perfectly harmonious with the methodological individualist's insistence that plans often miscarry (and that even when they do succeed, they almost invariably have other important and unanticipated effects). The methodological individualist only insists that the social environment by which any particular individual is confronted and frustrated and sometimes manipulated and occasionally destroyed is, if we ignore its physical ingredients, made up of other *people*, their habits, inertia, loyalties, rivalries, and so on. What the methodological individualist denies is that an individual is ever frustrated, manipulated or destroyed or borne along by irreducible sociological or historical *laws.*

The social scientist is, here, in a position analogous to that of the Cartesian mechanist.[1] The latter never set out to discover new and unheard-of physical principles because he believed that his own principle of action-by-contact was self-evidently ultimate. His problem was to discover the typical physical configurations, the mechanisms, which, operating according to this principle, produce the observed regularities of nature. His theories took the form of models which exhibited such regularities as the outcome of 'self-evident' physical principles operating in some hypothetical physical situation. Similarly, the social scientist does not make daring innovations in psychology but relies on familiar, almost 'self-evident' psychological material. His skill consists, first in spotting the relevant dispositions, and secondly in inventing a simple but realistic model which shows how, in a precise type of situation, those dispositions generate some typical regularity or process. (His model, by the way, will also show that in this situation certain things cannot happen. His negative predictions of the form, 'If you've got this you can't have that as well' may be of great practical importance.) The social scientist can now explain in principle historical examples of this regular process, provided his model does in fact fit the historical situation.

This view of the explanation of social regularities incidentally clears up the old question on which so much ink has been spilt about whether the so-called 'laws' of economics apply universally or only to a particular 'stage' of economic development. The simple answer is that the economic principles displayed by economists' models apply only to those situations which correspond with their models; but a single model may very well correspond with a very large number of historical situations widely separated in space and time.

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[1] I owe this analogy to Professor Popper

In the explanation of regularities the same situational scheme or model is used to reconstruct a number of historical situations with a similar structure in a way which reveals how typical dispositions and beliefs of anonymous individuals generated, on each occasion, the same regularity.[1] In the explanation of a unique constellation of events the individualistic method is again to reconstruct the historical situation, or connected sequence of situations, in a way which reveals how (usually both named and anonymous) individuals, with their beliefs and dispositions (which may include peculiar personal dispositions as well as typical human dispositions), generated, in this particular situation, the joint product to be explained. I emphasise *dispositions*, which are open and law-like, as opposed to *decisions*, which are occurrences, for this reason. A person's set of dispositions ought, under varying conditions, to give rise to appropriately varying descisions. The subsequent occurrence of an appropriate decision will both confirm, and be explained by, the existence of the dispositions. Suppose that a historical explanation (of, say, the growth of the early Catholic Church) largely relies on a particular decision (say, the decision of Emperor Constantine to give Pope Silvester extensive temporal rights in Italy). The explanation is, so far, rather *ad hoc:* an apparently arbitrary *fiat* plays a key rôle in it. But if this decision can in turn be explained as the offspring of a marriage of a set of dispositions (for instance, the Emperor's disposition to subordinate all rival power to himself) to a set of circumstances (for instance, the Emperor's recognition that Christianity could not be crushed but could be tamed if it became the official religion of the Empire), and if the existence of these dispositions and circumstances is convincingly supported by independent evidence, then the area of the arbitrarily given, of sheer brute fact in history, although it can never be made to vanish, will have been significantly reduced.

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[1] This should rebut Gellner's conclusion that methodological individualism would transform social scientists into 'biographers *en grande série*' (op. cit. p. 176).

WHAT MAKES A SUBJECT SCIENTIFIC?*

W. B. Gallie

I

When faced with this kind of question we are naturally subject to the contrary pulls of two strong philosophical tendencies, the one predisposing us to believe that our question must allow of a single definite answer, and the other predisposing us to doubt or deny this. The following arguments and counter-arguments, which I ascribe to spokesmen *A* (affirmative) and *N* (negative), develop the two views.

A We all commonly distinguish, within intellectual subjects, those that are scientific from those that are not. (The latter include some which are too limited or too indefinite to rank as scientific, and others, e.g. literary and art criticism, which have highly developed but definitely non-scientific aims and standards of their own.) Now if in making this distinction we know what we are doing, or what we mean, it must be possible for us to pick out some characteristic, or set of characteristics, which applies to all scientific subjects *and to no others*. To be sure, this job may not prove easy: and since the different sciences are constantly advancing and changing in respect of their mutual relations, and new sciences keep on arising, this part of the philosophy of science requires constant revision. Moreover, the defining characteristics of a science must be expected to be of a highly abstract kind, since the different sciences have very different subject-matters, involve different special procedures, and stand at very different levels of development. It would be foolish to expect the characteristics in question to be of a kind we can point to in separate cases: as though because a man was making measurements or using a slide-rule or setting out his results in some standard form, he must *therefore* be doing science. Nevertheless, for the reason already given, the required characteristics must exist and it is important for us to discover and describe them.

N The above argument is far from conclusive: if it were, then

* Received 12. vi. 56

for exactly similar reasons we must be able to define, e.g. what farming is in terms of characteristics that are common to all farmers and to no one else. But there are no such characteristics ; there are no activities in which (*a*) all farmers and (*b*) no one except farmers, engage. Thus (*a*) some farmers are dairymen, others not ; some raise crops, others do not ; some keep tractors, others hire them ; some fill up forms, others employ accountants to do so, etc. Again (*b*) many farmers keep poultry, but so do some miners and some clergymen ; many farmers hedge and ditch, but so do some council employees ; and so on. It might happen to be true that at some given time there is some one activity which all living farmers are engaged in, but this would be a contingent fact in no sense deducible from the way we commonly employ the words ' farm ' and ' farmer '. As far as ' the logic ' of our ordinary use of these words goes, the position might well be that, if there were only four farmers in existence, their respective activities could be set out as follows :

Farmer 1 engages in activities A, B, C
,, 2 ,, ,, ,, B, C, D
,, 3 ,, ,, ,, C, D, E
,, 4 ,, ,, ,, D, E, F

This shows that ' to farm ' means simply to engage in a certain number of (perhaps to engage in any selection of) a wide variety of activities, many of which stand in obvious economic relations one to another, but others of which are usually conjoined more because of convention and tradition than because of any definite economic design. Hence, if we wish to define ' farming' in terms of characteristics, as opposed to overlapping resemblances, we can do so only in terms of a disjunction, not in terms of a set of characteristics that is common to, and belongs exclusively to, all farmers.

Now, if we con over the various sciences, we see that they are in precisely this position. Different subjects, and achievements within different subjects, rank as scientific for reasons that are proper and peculiar to themselves. Is a new form of mathematical proof, or a new physical theory, accepted as scientific for anything like the same kind of reasons as render a particular measurement or observation or classification, say in botany, ' scientifically acceptable ' ? Or let us ask what makes us accept a certain new kind of cure in either physico- or psycho-therapy, or makes us claim that certain economic policies are scientifically justified while others are not ? The reasons, the considerations, the criteria which are relevant in these last cases are in some ways notably different from those that apply in the case of sciences whose aim is simply to classify and to predict. What makes one subject scientific may be something vastly different from what makes another scientific: just as, notoriously, very different gifts and training are required to make a mathematician, a physicist, a diagnostician, a botanist, an economist. True, scientists in all these fields observe; but so, notoriously, do painters, detectives, and others. True, scientists in all these fields present their conclusions on carefully gathered evidence; but so do historians and lawyers. True, finally, their conclusions take the form of general statements; but so do the conclusions of philosophers.

For these reasons, it seems clear, the words ' science ' and ' scientific ' do not stand for a single definable concept; and hence the question that forms our title does not admit of a single definite answer.

A The above counter-argument indicates a gap in the affirmative view as previously presented. But this gap is easily filled, and thereafter the affirmative view can be re-established without difficulty. What has been pointed out is that the word ' science ' suffers from the following ambiguity. At its widest it stands for almost any intellectual activity, with perhaps the proviso that its conclusions must be of a general character. Let us say that the word ' science ' thus understood stands for an *order* of activities. Under this order we find a number of sets of *genera*, obtained by applying a number of different but by no means incompatible systems of classification, which we adopt for different purposes: thus we have. e.g. the generic distinctions of inductive and deductive (or empirical and *a priori*), of pure and applied, of the *Natur-* and the *Geisteswissenschaften*, etc. Now any species of any one of these genera can be described as a science, on two possible grounds, first, as falling under its own proper genus, e.g. as being a deductive science (like algebra) or an applied science (like mechanical engineering), and secondly also, as falling ultimately under the order ' science ' in the widest possible sense of that term.

We can admit that the boundaries of this order are vague: that the term ' science ', in its widest sense, is a slippery one, used for the most part for dubious honorific purposes. But this is not true of the various generic distinctions that fall under it; and in particular it is not true of the all-important inductive/deductive (or empirical/*a priori*) distinction. We know quite well what we mean by an empirical or inductive science; it is easy in practice to distinguish any such science from, e.g. any purely formal (mathematical) science on the one hand or any purely descriptive study on the other ; theoretically, therefore, it must be possible to state accurately the *grounds* of these distinctions. Now this is of particular importance for our present discussion, since the question ' What makes a subject scientific ? ' would today generally be understood to mean ' What makes us accept a particular subject as an empirical science, or as one of the *natural* sciences ? ' Admittedly, in certain contexts, our question might be taken to refer to the criteria not for belonging under the genus ' empirical science ', but for belonging under one of the other genera mentioned above. The vital point is, however, that it would have clear sense only if it were understood to be referring definitely to *one* of these, not indefinitely to *any* of them. And granted this, there is no reason whatsoever why a single definite answer should not be given to our question.

We may agree with *N*, therefore, that if we were to bundle together *Naturwissenschaften* and *Geisteswissenschaften*, empirical and *a priori* sciences, pure sciences and applied sciences, sciences that study functions and sciences that seek for causes, we should arrive at no clear, well-defined concept of science. But the necessary distinctions to remedy this situation are to hand. At what we have called the generic level, i.e. in terms of some *one* of the main genera that it has been found useful to distinguish, the question of our title should admit of a clear and definite answer.

Finally, if a parallel be wanted for the logical situation that has been ascribed to the terms ' science ' and ' scientific ', a much more appropriate one than that of the term ' farming ' would be that of the terms ' king ' and ' kingship '. We all know that at different times, conforming to different levels of civilisation, ' kings ' have been magical priests, chosen war-leaders, supreme national executives, and hereditary constitutional civil servants. As with the word ' science ' when taken in its widest sense (to stand for an *order* of intellectual activity), so here, it is perhaps impossible to set any clear limits to the meaning of ' king ' if this be taken to cover all or any of the above different cases. It would therefore be pointless to look for politically interesting points of similarity and contrast between, say, any Scandinavian sovereign of the last fifty years and any of those ancient kings of Nemi described by Frazer at the opening of the *Golden Bough*. On the other hand it might well be profitable for an historian to institute a careful comparison between members of the former group, and for a constitutional lawyer to try to define precisely in what their ' kingship ' lay.

N Spokesman *A* now in fact admits that no single definite answer can be given to our question. But he proceeds to draw the lines between different possible answers in the wrong way.

If his account is to be believed then we never describe or appraise a result simply as ' scientific ', but always (in intention if not in actual words) as ' inductively scientific ', say, or as being a piece of ' pure ' as opposed to ' applied ' science. Moreover, on this new account, no useful purpose can ever be served by comparing a procedure or a result occurring within *one* of these *genera* with a procedure or result occurring within another genus of scientific work. Both these conclusions seem to be false. We group together certain sciences as empirical and others as *a priori* primarily, if not solely, on the ground that the conclusions of any of the former require confirmation from experienced fact, whereas the conclusions of the latter do not : hence important differences as to the meanings of proof and truth in the two groups of sciences. But the existence of these points of resemblance between, e.g. (i) botany and physics as empirical sciences, and (ii) arithmetic and formal logic as *a priori* sciences, and these points of contrast between the members of (i) and the members of (ii) do not preclude the possibility of other points of resemblance, perhaps of equal significance for an understanding of their scientific character, between a member of (i) and a member of (ii), or the possibility of other points of contrast, of perhaps equal significance for the same purpose, between the members of (i) or the members of (ii). These possibilities would be precluded if and only if the terms ' *empirical* ' and *a priori* stood for generic *essences* that set their generic stamp on every feature of every member of their respective species—as it might be imagined (though falsely) the characters ' male ' and ' female ' set their stamp on every component cell of every living human body.

The empirical/*a priori* distinction relates, then, to the *logical status* of different scientific conclusions. But suppose we are interested to compare and contrast a number of sciences, not in this respect, but in respect to (*a*) their characteristic methods of approach to problems, or (*b*) what I shall call the significance or ' surprise-value ' of their results, or (*c*) the inherent progressiveness of scientific research which they serve to illustrate. Are we, in any of these cases, precluded from crossing the boundaries of the opposed genera, empirical and *a priori*, pure and applied, etc., as spokesman *A* maintains ? It can easily be shown that we are not.

(*a*) *Characteristic methods of approach.*[1] A physicist makes a discovery by predicting correctly how certain bodies will move or come into equilibrium, given certain initial conditions : a taxonomist makes a discovery by finding a new specimen *and* its place in his classificatory scheme. Both are discoveries within the field of empirical science, and as such are to be contrasted with any newly discovered conclusions (theorems) in logic or pure mathematics. Yet if we consider the approach required in the two cases—not to mention the different gifts, training, experience, etc., that are required—it is clear that the physicist's methods show a far greater resemblance to those of a mathematician than to those of the taxonomist. Physicist and mathematician alike begin by seeing a *problem* (almost like seeing a gap, an absence) whereas the taxonomist usually begins by seeing, hitting upon, something positive, which yet no one expected to be there. Moreover, to see a problem is—alike for physicist and mathematician—to see the need of a solution, a statement deducible from certain highly general principles or postulates ; and here again there is no real resemblance to the taxonomist's case. Again, physicist and mathematician alike proceed to employ, it would appear, two main sorts of procedures which may be described as (i) reduction of the problem, initially, to the simplest possible form—to a point of abstraction that may make it (to the layman) almost unrecognisable, and (ii) a persistent juggling with, or re-arranging of, the terms involved, often under the guidance of other partially parallel cases, until a possible solution is perceived. These methods of approach seem to be appropriate wherever the making of discoveries is in the nature of problem-solving : but, as we have seen, by no means all discoveries in the empirical sciences are of this kind.[2]

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[1] To be distinguished clearly from characteristic methods of *testing*. On the relation of these two criteria of ' the scientific ' see section 2 p. 125.

[2] Nor should it be thought that our description applies to the physicist's methods of approach simply in so far as he makes use of mathematics, geometry or algebra, in order to formulate his hypothesis or proposed solution. Some of the simplest theorems in classical mechanics (as similarly in Euclidian geometry) admit of being intuitively ' seen ' once the initial conditions are understood : but they are seen as ' what would inevitably follow ' given the general character of the system : and a mechanically gifted child or savage, possessing no mathematics whatsoever, might well look along or follow the mechanism of the system in the expectation of the same kind of result (but envisaged in qualitative terms) as the physicist's proof sets out with mathematical rigour and accuracy. Of course it is true that there are many physical discoveries which could not conceivably have been made but for the use

(*b*) *Significance or ' surprise-value ' of results.* Often we describe a result or discovery as scientific because of the quite surprising degree to which it surpasses our pre-scientific or ' commonsense ' capacities and corrects or transforms our pre-scientific beliefs in some particular field. In particular we recognise the emergence of a *new* science when, by whatever means, it enables us to obtain results which in respect of accuracy, range, generality, flexibility, manageability or what not, make our pre-scientific knowledge of the matter in hand look barbarous or infantile. A piece of knowledge counts as scientific in this sense because it is something of a miracle, something altogether special —which yet, and this makes it all the more miraculous, admits of a perfectly intelligible explanation. Now when we use the word ' scientific ' in this sense, in which appraisive and descriptive features are subtly interwoven, it is certainly open to us to make comparisons which cut clean across the divisions of empirical/*a priori*, pure/applied and so on. It would be perfectly legitimate to compare the twentieth-century revolution in psychotherapy (a science of cure and not in the ordinary sense an applied science) with similar revolutionary phases in any of the pure sciences, mathematical or empirical. Significance or surprise-value is, thank goodness, not a perquisite of any one department of human knowledge.

(*c*) *Inherent progressiveness.* Ever since Kant emphasised it, the apparently inherent *progressiveness* of the sciences has fascinated philosophers. For our purpose it is sufficient to recall that scientists treat most, if not all, of their principles, theorems, and observations as fallible—as almost certainly capable of being rendered more accurate or of being reformulated to cover instances which at first sight appear to contradict them. Moreover, it is characteristic of scientists to greet such apparent contradictions, not with irritation or regret, but with a surprising eagerness to hear the worst, since this may well indicate the path to further discoveries. Here indeed is one main condition of that ' inner dynamism ' which keeps every science moving forward.

This inner dynamism we are prone to attribute exclusively to the

of mathematical analysis : but it is equally true to say that the character of physical problems that we have just described helps us to understand why mathematical solutions are relevant to them at all. The child or savage with a flair for machinery but no mathematical knowledge forms his expectations by performing crude feats of ' qualitative analysis ' that offer us a blurred adumbration of the mathematical physicist's procedure, and certainly show no important resemblance to the acts of fine discrimination and the mastery of some immensely complex classification system that are characteristic of the taxonomist.

'pure' sciences. The pure scientist, we say, wants to learn what the laws of nature *really* are. The applied scientist, by contrast, is interested in the laws of nature simply in so far as they are relevant to the efficiency of his particular projects, constructions, machines, etc. Not the ultimate truth, but the truth that stands the test of practice here and now is what interests him; and in this respect he is like every other practical man. A keen 'nose' for possibly significant snags or inaccuracies or for further generalisations of principles he is using: an eagerness to see precisely where and to what extent a machine's performance or a crop's yield has surpassed expectations—these attributes should therefore be quite out of place in the applied sciences. But of course one has only to say these things to realise how absurd they are. And at once we recall how many of the most crucial advances in the pure sciences have owed their impetus and direction to acute and prophetic observations made by 'practical men' in the course of 'applying' theoretical principles. Finally, we may recall what we have read of a number of growing-points in contemporary science, at which it is virtually impossible to distinguish the work of the scientist who is a pure experimentalist from the kind of scientist who (here evidently in no pejorative sense) 'is really an engineer'.

And now to apply these examples to the main discussion. The suggestion of spokesman *A*, that the well-known classifications of the sciences, into empirical as against *a priori*, or into pure as against applied, etc., serve to indicate 'real' or 'essential' divisions within scientific knowledge or activity, must be rejected. Quite the contrary, some of the most illuminating parallels between the different sciences can be made *across* these divisions: and some of the most serviceable criteria of 'the scientific' require that we shall disregard them. There is therefore no force in the last argument by which *A* purported to show that our question 'What makes a subject scientific?' admits of a single definite answer.

2

At this point we may bring the dialogue portion of this paper to a close. Dialogue is a useful form of philosophical expression when the subject at issue is sufficiently familiar and clarified; but, with regard to the main positive point I want now to try to develop, the prevailing thought-habits of today are not near enough to what I want to say for the thrust and counter-thrust of dialogue to be of assistance. What

I wish to develop is a challenge that might be put to spokesmen *N*, namely: In your view, *do we possess a concept of science at all?* And, on the supposition that *N*'s answer to this continues in the negative, I proceed to suggest an account of the structure of the concept of science which takes full account of all the complicating factors mentioned by *N* in his criticisms of *A*, and indeed arises out of a close inspection of the relation between our different criteria for the use of the word ' scientific '. But before I undertake this task I must say something on two preliminary questions: (i) Are there any other important criteria of ' the scientific ' besides those mentioned in the previous discussion between spokesmen *A* and *N*? (ii) Can we state, in schematic fashion, the relations of mutual dependence (and/or independence) between what appear to be the most important different criteria of ' the scientific ' ?

As to question (i), I can think of only two other serious candidates, of which the first is the *publicity* criterion. Two distinct but connected things are, I think, commonly meant by this and kindred phrases: (*a*) that no observation counts as scientific unless it is available, directly or indirectly, to any normal and unbiased observer: and (*b*) that, to ensure this result and also for more positive reasons, scientists seek, wherever possible, to state their findings in a form so generalised that any competent and unbiased inquirer can give them an interpretation (no doubt in some cases only a very indirect interpretation) in terms of his own sense experiences. Thus understood, does the publicity criterion add anything of importance to the criteria that have figured so far in our discussion?

I have no doubt that the publicity criterion is of very great importance not only for understanding what ' science in general is ', but for appreciating the point of many particular scientific procedures and expressions. I think, however, that it is unnecessary to add it to our list, since it would be both natural and more economical to assume that each of the criteria that we have mentioned or discussed so far itself involves the publicity criterion. Thus I would urge that, by definition, a proof—and likewise the appropriate tests for a proof's validity—is subject to the conditions (*a*) and (*b*) above which the publicity criterion contains. Similarly, I would say that any methods of approach that are appropriate to a given scientific problem must, by definition, be theoretically accessible to all intelligent students of that problem. Again I would urge that a very important *direction* in which ' significance ' or ' surprise-value ' is commonly sought by scientists is that of maximising the public character of their observations and theories: indeed we have here one aspect of the scientist's endless quest for increased generality. And somewhat similar remarks could be made in connection with the alleged ' inherent progressiveness ' of science and its ' inner dynamism '.

The second further criterion which deserves to be discussed relates to scientists' motives or intentions: we may call it the criterion of *disinterestedness* or *impartiality*. Certainly we often hear these aims—and the correlated attitudes of mind—described as essential attributes of scientific work. But not only are these aims equally proper to all other serious forms of intellectual activity: it seems to me that, anyhow on some occasions, scientific work may be advanced without them and the attainment of scientific truth may actually be accelerated by the passionate, and even the egotistic partisan bias of researchers in favour of their own chosen methods or theories. This would indeed have sounded shocking, if not utterly absurd, to any good scientific man of the seventeenth or eighteenth century. But today we all know that for the last hundred odd years scientific standards—including standards of objectivity—have come to be ensured more and more by various scientific *institutions*. In particular, so long as the judges of scientific work are impartial, it is quite possible that an acceptable solution to some problem will be achieved sooner if different competing researchers ' attack it ' following ' with passion ' their own favoured approaches. On the other hand, there are unquestionably other respects in which the criterion now being discussed is of the first importance to all scientific work; but in these respects I believe that its import is covered by my criteria (ii) and (iii) as discussed below.

In connection with our second question—as to the mutual relations of our suggested criteria of ' the scientific '—I should like to say here a little more about those resting on the ideas of ' logical status ', ' methods of testing ' and ' methods of approach '.

(i) The phrase ' logical status ' signifies the first conception of ' the scientific ' to emerge in western thought. What struck ' Thales or some other ' with so forcible a light was presumably that certain of his conclusions could be *proved*, i.e. geometrically proved. It was this which distinguished them from the general mass of mankind's habitual or occasional beliefs; and it was this that made Thales' discoveries marvellous to responsive fifth-century Greek minds. Moreover, it was the same feature of *demonstrability*, as is well known, that dictated the meaning of ' scientific ' that we find subsequently in Aristotle and in St Augustine and in Descartes, and even in Locke. Later, of course, it was seen by Hume and others that conclusions deserving the title 'scientific' could be of *more than one* logical status—i.e. they might be empirical conclusions so formulated as to be in principle falsifiable, and yet in fact unexceptionally confirmed by experiment; and it might well be urged that other 'logical-status-dimensions' might today usefully be added to the powerful empirical/*a priori* division. Be this as it may, however, what will concern us below is the relations between logical status, *of whatever acknowledged kind*, and the methods and results and other characteristic features of the sciences.

(ii) What precisely is the distinction between 'methods of testing' and 'methods of approach'? The two notions, and the contrast between them, are familiar enough, yet they are very seldom grasped aright. The following very simple parallel seems to me to show the essentials. It might be said that there are three methods, or more colloquially three ways, of getting a batsman out L.B.W. in cricket, and this might naturally be taken to mean (*a*) by means of a dead straight ball due to hit the wicket, (*b*) by means of a ball pitching on the wicket and, although breaking slightly, still due to hit it, and (*c*) by means of an off-breaking ball which, even if pitched off the wicket, can be *seen* to have been due to hit it. But alternatively we might speak of three methods or ways of getting batsmen out L.B.W. and mean or refer to three major lines of strategy by which bowlers, capable of spinning the ball, can trap batsmen into getting out in this way. In the former sense there are three tests for L.B.W.; in the latter there are three approaches or strategies. Now, reverting to science, it is quite clear that methods, in the sense of methods of testing, have a very close connection with the logical status of the conclusion to which they apply; whereas the connection between any one method of approach or strategy and a consequent successful result is not of a logically necessary, although still of a very intimate, kind. Indeed, it seems to me, that what is of value in the Verification Principle could have best been stated as follows: To understand any scientific statement R in a manner that is adequate to its scientific status, necessarily involves some degree of understanding of the methods of approach which suggest, and make possible, the use of certain methods of testing R. Thus, for example, if one is to appreciate the cogency of, or experimental test of a scientific formula, one must clearly have *some* understanding of how the initial conditions of that experiment serve to represent the whole range of phenomena referred to by the subject-term (or protasis) of the formula in question ; and to understand this involves some understanding of the method of approach that led to, or made possible the choice of, the experimental test in question.

We are now in a position to deal with the question of the mutual relations of our five suggested criteria of ' the scientific '. And I propose to set these out through answers to the following questions, viz. is any of the proposed criteria self-sufficient, i.e. such that by answering to it alone a subject or investigation can properly be described as scientific ? Or, on the contrary, is none of them sufficient in this sense ? And in that case is some disjunction of them sufficient ? Again : Is any one of them necessary to any proper description of a subject or investigation as scientific ? Or is any disjunction of them necessary ? Let me go through the list of our suggested criteria with a view to settling these questions.

(i) *' Scientific ' as signifying some definite logical status of a conclusion or other formula.* Evidently this sense of ' scientific ' (or alternatively this suggested criterion of ' the scientific ') cannot be self-sufficient, if to accept, e.g. a conclusion as ' scientifically proved ' involves knowing its appropriate tests and also some appreciation of the methods of approach that have led to, or suggested, these tests.[1]

Is then this sense of ' scientific ' necessary ? Again the answer seems to be No, for all that the sense in question has Thales and and Aristotle for its fathers. It seems to me that there are certain results, and equally certain principles or postulates, which we would all (or most of us) *today* unhesitatingly describe as scientific, although their exact logical status is almost anyone's guess—certainly there is nothing like ' expert unanimity ' about them. This seems to me true, e.g. of many of the key doctrines of Freudian psychology—all of them so inextricably bound up with the logically baffling notion of ' cure ' ; and equally true of some of the most general principles or postulates of the physical sciences, e.g. the laws of conservation. (Are these latter to be accepted, and do they function, as *a priori* or as empirical laws : as postulates or as inductively established truths ?)

(ii) *' Scientific ' as meaning ' subject to definite methods of testing '.* Evidently this sense of ' scientific ' (or criterion of ' the scientific ') is not sufficient if to know that, e.g. a principle is to be tested in *this* or that way involves in every case some appreciation of a requisite ' method of approach '.

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[1] It is of course possible to accept (to believe or know) *on authority* that a given statement has some logical status, without knowing the appropriate related methods of testing and approach. But such belief or knowledge would be, *by definition*, non-scientific.

Is it necessary ? Here we meet a puzzle which the case of Freudian teaching again aptly illustrates. We feel we must say : No definite tests, no scientific results. But if we know (adequately) what the tests are, then surely we must know (adequately) the logical status of the results they enable us to achieve. But we do not, in the case cited, know the latter adequately ; how then can ' Freudian diagnosis ' be subject to proper tests and therefore count as scientific ? I suspect the answer to this must be : that while, in claiming a scientific character for a certain theory, we are committed to asserting the *existence* of appropriate tests, it is nevertheless consistent to deny that anyone possesses an adequate *description* of them, and that it is for this reason that the logical status of our conclusion remains in obscurity. If this is acceptable, then the answer to the question ' Is it necessary ? ' is here Yes, but subject to the enormously important qualification that we can't say exactly *what* we are claiming to be necessary.

(iii) ' *Scientific* ' *as meaning* ' *the use of certain definite methods of approach* '. Evidently this sense of the term is not sufficient. A pedantic ass, or a conscientious researcher up a blind alley, may employ methods borrowed or adopted from other inquiries ; but if his use of them is unintelligent or intrinsically in vain, or if his results are trivial we should not call his work scientific *simply because* it repeats certain standard methods of investigation.

Is this sense of the term necessary to (or in) any proper ascription of the word ' scientific ' ? I think it is, and for the reasons that can be illustrated as follows. Suppose a freak genius, either in mathematics or in natural science, who always proposes conclusions acceptable under the most rigorous generally acknowledged tests, but can never explain even in outline how he ' got his ideas ' ; suppose in fact he claims to get them from ' voices from the other side '. Would we accept his conclusions as *scientific ?* Only, I suggest, on the hypothesis that they came ultimately from a source that could explain how they had been reached by *him*, and explain this in such a way that any intelligent reader could, by some kind of analogy, appreciate the methods of approach that had led their original ' source ' to accept them.

(iv) ' *Scientific* ' *as signifying* ' *the achievement of significant or surprising results* '. Is this sense of the word self-sufficient ? Evidently not, if we were right in allowing that it is necessary, for a result to count as scientific, that some account be given of the methods, both of approach and of testing, that lead to its acceptance. Is it necessary? Again the answer must be No, for all that we often argue, especially polemically (and in certain contexts justifiably) *as if* it were so. This happens whenever we reject the scientific claims of an investigation with the question ' What new or illuminating or more-than-common-sensical results has it to offer?' But however justified in particular cases, criticism of this kind is not always admissible. If it were, then the first crucial steps in (or towards) any new science could never be said or seen to be of genuinely scientific character: and this is surely absurd. Again, if the production of significant, surprising results were a necessary condition of scientific work, there could be no such thing as work which contributes to science by revealing difficulties or apparent inadequacies in currently accepted theories; and certainly work of this kind is frequently and rightly regarded as scientific.

(v) *' Scientific ' as signifying ' inherently progressive investigation '*. Is this sense of the word self-sufficient? The answer must be No, for the reason advanced, on this question, at (iv) above. Moreover, despite the great importance of this characteristic of scientific work, it cannot be claimed to be necessary to any particular branch of science. For the scope of any such branch—say of physics, to take what is at first sight the least favourable case—may conceivably prove to be finite: witness the apparently absolute limitation upon observation and prediction which physicists seem to have encountered in at least two directions of their work. And whenever a field is finite, progressive investigation of it may some day come to stop.

We have thus reached the following result. None of the above senses of ' scientific ' (or alternatively none of the related criteria of ' the scientific ') is self-sufficient: and only two of the above senses (or related criteria)—and one of these with a very odd qualification attached to it—is necessary, to any proper use or ascription of the word. On the other hand it is not difficult to see that the conjunction of our two necessary senses (' scientific ' signifying ' subject to definite methods of testing and of approach ') with a number of disjunctions of our other senses of the word, will give us a self-sufficient sense of it (or a sufficient criterion of its proper use): viz. senses (ii) and (iii) above conjoined with *either* sense (iv) or sense (v). Moreover, I think it could be shown that various disjunctions of our sense of ' scientific ' other than (ii) and (iii) are also necessary to any proper use of it: viz. either sense (i) or sense (iv) or sense (v). Now of course my choice of these five senses (or criteria) might be criticised: other additional suggestions could no doubt be made, or our different uses of the term ' scientific ' might be grouped in a somewhat different way. But no-one, I think, can reasonably deny that there is a marked variety in our uses of the term in different kinds of context; certainly anyone acquainted with the history of science must agree that its pages disclose a surprising variety of suggested criteria of ' the scientific '. Moreover, I think that, no matter how our different senses of (or criteria for) the term were grouped, something very like the above relations of mutual dependence would be found to hold between them: viz. no senses (or criteria) that are self-sufficient and only a few that are actually necessary to any and every use of the term.

Now this situation is in some ways surprising to the point of absurdity. How can we talk of so many different *senses* of a term when on close inspection each of the alleged senses turns out to be dependent upon some or all of the others? Yet, on the other hand, it seemed perfectly natural to talk in this way: and in support of it, we may recall that each of our five criteria of ' the scientific ' has, at some time, been acclaimed as *the* (at least sufficient) criterion, or as expressing the essence, of scientific work by intelligent and knowledgeable philosophers of science. Are we then to leave the matter here: to rest content with showing that the application rules of the term or concept ' science ' are of the untidy sprawling kind that our spokesman *N* suggested, and that our later closer analysis seems to have confirmed? I am not at all satisfied to leave the matter like this. It seems to me incumbent upon philosophy, in the case of any ' logically obscure ' concept to explain why it is obscure, to explain why it has its characteristic structure or lack of structure. But how is this to be done?

3

Suppose we had an ideally adequate historical appreciation of the beginnings, growth, and proliferation up to today of all ideas and procedures and results that might by any reasonable standard be termed ' scientific '. Then, *eo ipso*, we should be in a position to understand the various relations of all the diverse possible currently accepted uses of the term. There is nothing odd or heterodox about this: it is true because platitudinously true. We can come to understand historically the proliferation of an idea only by knowing or inferring the kinds of consideration that led its users, at a given stage in its history, to develop it, say by bifurcating it or by associating it with other ideas. Its history, therefore, is the history of its successive uses, or of the successive stages of its developing use ; and since, on the present hypothesis, our history of scientific ideas is carried up to the present, it must include an understanding of the relations of the correct, currently accepted uses of the term ' scientific '. Admittedly, such ideally complete historical knowledge belongs to no man ; so that our hypothesis must remain—like all hypotheses of perfect knowledge of matters of fact—inevitably void, essentially unfulfillable. Nevertheless I would maintain that it indicates the kind of framework into which any satisfactory answer to our present question must be fitted.

Working on this supposition, let us therefore ask : In the light of what sorts of consideration would we expect the proliferation—or as it might be the restriction—of any scientific idea to be historically explained ? Without attempting to enumerate them exhaustively, we can be confident that considerations of a number of different sorts will here be relevant, e.g. the need to adapt laws or theories to cover negative instances ; the urge simply to generalise a particular theory (irrespective of negative instances to it), or the urge to unify—or as it may be disentangle—a number of theories ; the urge to greater and greater refinements of measurement ; the natural tendency to retain and improve intuitive clarity in symbols and theories, or the need to sacrifice these in the interests of greater generality, coherence, etc. To mention considerations such as these is to point to the fact that any particular science can be advanced in a number of different ways, and for this reason prized or valued on a number of different grounds. Every fresh development that a given science shows can, therefore, be regarded as a further articulation of at least one of the main reasons for which it has hitherto been pursued, and for which it has been valued ; (although of course when we regard any science from this latter point of view we naturally speak in terms not of needs, urges, or spurs to further improvements, but of the marks of advances actually achieved : we speak, e.g. of the rigour of proofs, the stringency of tests, the comprehensiveness of theories, the adaptability or suggestiveness of symbolisms). In sum, properly to appreciate (in the theoretical sense of that word) the main lines of growth of a particular science is also to appreciate (in the appraisive sense of that word) those features of it that have made and continue to make men prize it and pursue it.

To bring out the force of this suggestion it will be useful to contrast it with what has been called the 'theorem and experiment view' of science. On this view any given science consists simply of its theorems, or other accepted results, together with the classic (accepted) tests for these and the classic (accepted) methods of approach by which these theorems and tests have been discovered. To be sure, such an account will always be accompanied by a gloss to the effect that the definitive theorems and experiments of the science in question may at any time have to be altered or replaced because of new discoveries. Yet, despite this gloss, the 'theorem and experiment view' suggests a degree of completeness and self-dependence—even if only temporary—in any science, which is curiously unrealistic. It suggests a picture of a science as a box of tricks or a battery of procedures for obtaining true general statements or other required results in the most machine-like, automatic possible way. Something is evidently lacking in this picture: something that is, anyhow in the first instance, more easily illustrated than named or described in general terms.

Let us imagine the classical theories of light being presented, with the necessary experiments and calculations, to a student who, although of good general intelligence, unfortunately just lacks those special aptitudes which are necessary for the successful study of physics. At the end of the demonstration, he claims to have followed everything all right; but then proceeds to ask: Why did we approach the problems in only this way? Why (perhaps) did we approach them in this way at all? Why shouldn't we have begun in some altogether different way, with altogether different postulates, and favouring altogether different kinds of experiment? (Roughly Goethe's questions.) Well, how *ought* a physics teacher to treat such a student? Harsh though it sounds, I believe that the right treatment would be to advise another subject of study: perhaps (as in Goethe's case) the psychology or the physiology of colour vision. For, whatever the value of the student's suggestions, it is virtually certain that what he wants to do 'isn't physics'. But what are the grounds of this judgment? Roughly they are that any particular physical experiment or piece of theory is an example of a certain style or *genre* of explanation that has been developed successfully to deal with a wide variety of phenomena—and developed not only in the way of internal unification of its theories, codification of its tests, etc., but in the way of an ever deepening and ever more firmly entrenched sense of its own proper possible aims. More simply, work in physics as in any other science has come to mean work done within a tradition.

But the tradition of any branch of science, and indeed the broader scientific tradition itself, is something of an oddity ; for it is a tradition of discovery, and of discovery through constant self-correction and subsequent free development. Let us therefore try to get a little clearer about traditions of this kind.

Commonly we mean by a traditional activity a sequence of actions carried out in conformity with a fixed model or norm whose requirements are communicated from earlier participants to later, e.g. from one generation to another. But this is only the most elementary and obvious form of traditional activity, within which free development can take place only by accident or oversight. Clearly a progressive tradition, such as the scientific tradition of methods of making ever new discoveries, is a much more complex affair. In marked contrast with such elementary traditions we have sequences of action which are properly accounted progressive, but within which there seem to be no real elements of tradition at all. This is liable to be the case with technologies : for instance the obviously progressive successions of methods of land-transport and of factory-organisation show few (certainly very few important) traditionalist features. But, as we have seen, the most developed sciences show marked traditionalist features in spite of their histories of constant self-correction and of radical (and sometimes even revolutionary) progressive development.

Under what conditions then, we must try to discover, can a markedly progressive activity or sequence of actions show a no less markedly traditionalist character ? It will do so, I suggest, only when a number of its later more complex and sophisticated stages stand in the following two-fold relation to some of its more basic or elementary stages : viz. (i) that these more basic stages are necessary preconditions of the later more complex developments, and (ii) that these latter can be regarded as articulating more fully certain values or aims already exemplified in or pursued by the former. But these two conditions, although suggesting the basic pattern of the scientific tradition, by no means indicate the whole story. The following further complications must be noticed. (*a*) Within a progressive-traditionalist activity a feature that was at an earlier phase necessary to certain later developments, may well at a later phase cease to be necessary for subsequent developments ; and conversely, a feature which was in a corresponding sense non-necessary at an earlier phase may later become necessary for future developments of the tradition. A specific case of this last possibility would be the growth into predominance, within a progressive-traditionalist activity, of an aim which earlier was only unconsciously held or else was consciously subordinated to another. (*b*) It is even possible that such an ' upstart ' aim should be taken eventually to define the traditional activity in question. This is unlikely to happen, however, if the tradition is (as the scientific tradition certainly is) of a many-stranded or internally complex kind ; for, to take our particular case, the different sciences are notoriously at very different levels of development, and at a time when one science has developed—from its elementary traditional roots—some highly sophisticated aim of its own, other sciences will still be content with aims such as seemed good to the earliest scientists we know of. In sum, work in different sciences will ' claim kin with ' and be accepted into the scientific tradition on very different grounds, and will enter and contribute to that tradition in very different ways at their very different levels. Yet this does not in any way impugn the reality, the peculiar unity, of the scientific tradition.

In applying the above somewhat abstract analysis to our main problem, I propose to concentrate only upon the fifth (alleged) criterion of scientific activity which we discussed above, viz. that of *inherent progressiveness:* it should then not be difficult for the reader to see the bearings of my thesis upon the four remaining criteria. In the case, then, of our fifth criterion, we may summarise the relevant historical facts as follows. The earliest indisputable scientists we know of, the Greek geometers, seem to have been entirely unacquainted with it. For them to do science meant to produce their demonstrations of the few demonstrable truths they had so far hit upon, and perhaps also to search around (or pray) that they might hit upon others of the same logical status, belonging to the same (one and only) infallible system. Nor had inherent progressiveness any place in Aristotle's—or for that matter in Bacon's or Descartes'—conception of science. On the other hand since early in the eighteenth century—and more particularly since Kant—acceptance of inherent progressiveness as the hallmark of scientific work has become steadily more widespread ; with the result that today science for most people on most occasions means ' research science ', the expansion and refinement of old truths, and the scientific spirit is commonly equated with the will to try-and-err-and-*learn* for ever and a day.

What, then, is the bearing of our ' traditionalist ' approach upon this situation ? And how does our approach help to explain our previous conclusion that, despite the high claims that have been made for it, inherent progressiveness cannot be regarded as logically either necessary or sufficient for the definition of scientific activity ? It helps in a very simple and obvious way, by reminding us that this alleged criterion arose relatively recently and in connection, in the first instance, with only a few highly developed branches of science, in particular Newtonian physics. Our traditionalist approach also serves to remind us that there remain at the present day plenty of more elementary (but highly respectable and often extremely difficult) forms of scientific work whose proper and natural criteria are (when suitable adjustments have been made) very much more like those that appealed to the Greek geometers or to Bacon. Ask any researcher in some still undeveloped branch of the biological or psychological sciences for the considerations or criteria that lead him to claim that his work is scientific. The answer will come pat in terms of results, whether proved or ' very probably significant ', of repeatable methods of approach, and of rigorous and exhaustive methods of testing. The idea of the inherent progressiveness of his own line of research will quite naturally not yet have occurred to him. In fact, it is a criterion (or quasi-criterion) that could be accepted *in practice* in only a very few, very highly developed sciences. A theoretical physicist (or perhaps also a theoretical biologist) may discard a theory because acceptance of it would appear to bring further self-correcting and developing theorising to an end. But this is a consideration that could only have weight in a science where there is already an immense accumulation of exact and highly organised knowledge, and in which there are to hand an indefinite number of alternative possible lines of further investigation.

And now we are in a position to state, and to appreciate, the really fairly obvious truth that the use of our fifth criterion was always ' persuasive ', in the way that certain definitions are. It was never even a plausible general criterion of scientific activity—of all the various many-levelled activities that fall within the scientific tradition.[1] But it did focus attention upon what appeared to be a unique and profoundly impressive feature of the most developed branches of science. In them the possibility of continuous self-improvement, which of course could be attributed to almost any activity as a mere abstract possibility, suddenly appeared as a visible and unmistakable trend ; a trend, moreover, which it was most tempting to regard as inherent to all scientific activity, since work done in a progressively self-corrective spirit certainly seems likely to produce more quickly progressive self-correcting results. In this way our fifth alleged criterion of scientific activity has had the effect of an encouragement to less developed branches of science to see themselves as capable—or as ultimately due to be capable—of manifesting the most remarkable feature which any branch of science has so far developed : in other words it works as a ' contestant '[1] thesis with regard to the scientific tradition, having the aim of influencing the attitudes of those who belong within the tradition and thus indirectly of determining its further future developments. Such is the explanation of our fifth alleged criterion to which our traditionalist approach naturally leads.

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[1] Other reasons, besides that argued on page 131, can be shown to support this conclusion : in particular (i) that progress in science evidently depends to a considerable extent upon favourable political and social conditions, and therefore cannot be regarded as inherent in science, and (ii) that if such progress were inherent in any particular science its methods would *eo ipso* exemplify a positive solution to the problems of induction—for how otherwise could these methods be guaranteed to achieve ever fresh successes in the indefinite future ?

But does this same approach also help us to understand the positive conditions of the growth of our ' quasi-criterion ', as we may now justifiably call it, in relation to the others we have listed ? I believe it does, although on this score what I have to say must remain brief and highly programmatic.

Consider the three following heuristic principles which came to be accepted by physical scientists during the seventeenth century. (i) All observed changes to be recorded in quantitative terms ; (ii) no metaphysical divisions within—or exclusions from—physical nature, so that conceivably any physical phenomenon may prove itself a ' retainer ', in Locke's happy phrase, to any other ; (iii) all classifications to be made on a nominalistic, or provisional or conventional basis. Now, for simplicity, let us assume that these principles were accepted by all influential seventeenth-century scientists for the same general reason : e.g. Cartesian, in the interests of clarity and distinctness, or Baconian, in the interests of predictive power. (In fact these reasons were often combined with others which we need not bother to disentangle.) The point I wish to make is that, for whatever reasons they were accepted, these did not include the requirement that physical explanations should prove themselves self-correcting and inherently progressive. Quite the contrary. And yet each of these principles naturally (although of course not necessarily) pointed the way to and contributed to the emergence of our fifth quasi-criterion in the eighteenth century. Thus (i) every method of measurement admits, as a logical possibility, if not in practice (and even if not on currently accepted physical theories) of indefinite refinement. (ii) As scientific theories connect more and more distantly related phenomena, our original descriptions of these phenomena are likely to stand in need of progressive revision. (iii) While a nominalist attitude to classifications will bring to an end all quarrels as to their real or essential basis, it will in no way discourage —quite the reverse—the search for more and more powerful systems of classification, whether uni- or multi-dimensional.

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[1] For a discussion of the usefulness of this way of thinking (though not with reference to the concept of science) see my ' Essentially Contested Concepts ', *Proceedings of the Aristotelian Society*, 1955-56, **56**, 167-198.

Our traditionalist approach does help us, then, to take the first step towards a clearer understanding of the relation between some of our quasi-criteria of 'the scientific': for it shows us three heuristic principles, which initially commended themselves to scientists concerned exclusively with our first three quasi-criteria, and which yet gave rise to the situation in which the idea of inherent progressiveness came to be expounded and canvassed. To be sure, our explanation is programmatic only: it needs to be made good in a vastly fuller, more pointed, more revealing account of how our fifth quasi-criterion came to commend itself so powerfully to scientists and to philosophers and other prophets of science. But in fact there is no call for apology on this score: for we are simply showing how the apparent deficiency of our original supposition of an 'ideally adequate historical understanding' can be remedied—in the recommendation and requirement of ever fuller, ever more illuminating treatments of that great central theme of the history of science, the making of the scientific tradition.

Question. What makes a subject scientific?

Answer. That it has in fact been, or that it is truly claimed to be due to be, accepted into the scientific tradition.

Question. How can this answer be rendered clearer, more illuminating, in any particular case?

Answer. Only through successive attempts to write the history of science in a more truly historical manner, i.e. so as to reveal on what grounds new results, methods, approaches, etc., have been or are being accepted into the scientific tradition.

Moral. Scientists, philosophers, historians, *above all historians of science*, to draw their own.

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DIATHESIS, THE SELF-WINDING WATCH, AND PHOTOSYNTHESIS *

H. A. C. Dobbs

1 *Diathesis and the Self-winding Watch*

In various recent publications Professor R. O. Kapp has put forward a new form of vitalism suggesting the need for an immaterial agency, called a ' Diathete ', to explain the characteristic behaviour of living things contrasted with that of ' lifeless ' machines, such as servo-mechanisms, however cunningly the latter may be contrived. This diathete is conceived to be a sort of entelechy, without location in space but capable of exerting a causal influence over physical structures, which are located in physical space. It follows of course that a diathete does not obey the laws of physics; since these laws apply only to processes and entities which are located in physical space.

Kapp was led to postulate the existence of this immaterial entelechy, the diathete, in connection with the peculiar behaviour of living structures, through observing that the most striking and characteristic feature of this behaviour is controlled and selective response on the part of an organism, to the random forces and influences exerted by its environment. In other words the peculiarity of a living thing is that it has the capacity to produce an orderly controlled output in response to a set of random stimuli which constitute the input. Kapp has put this point in the form of a question : ' Can random forces be caused to produce a specific event at specified moments of time ? ' Kapp's answer is that no inorganic structure, such as a lifeless machine, can be devised which will respond to an input of random forces and disorderly influences with a controlled output, without the intervention and assistance of a non-physical entelechy, the ' diathete '.

The primary object of this paper is to refute Kapp's contention, that it is necessary to introduce a non-physical agency of this kind, in order to enable random forces to produce controlled movements and

* Received 28. vi. 56

specific events at specified times. I shall do this by citing a particularly simple example of an inorganic material mechanism, which is completely subject to the laws of physics, but which none the less contrives to use random forces so as to produce controlled movements.

In considering Kapp's theory one is mainly concerned with *the process resulting from a diathete's operations*, which process he calls '*diathesis*', rather than with the nature of a diathete in itself. Diathesis he suggests: 'As a term that would cover both input function and output function, that would cover indeed every process calling for control, guidance, selection, discrimination, timing.'[1]

Kapp conceives diathesis as operating in all living organisms between the input and the output. A diathete operates causally; but the causal laws governing its operations are not the laws of physics. For example in the case of the human organism he says: 'the particular link in the chain of causation called *Mind* does something other links do not. It introduces diathesis. It does so by selecting from the unco-ordinated mass of sense-data those that make up an appropriate stimulus for a given response; by exercising guidance, control by introducing specified order into the course of events.'[2] In other words, a diathete operates in the input of human sense-perception as a detector which selects, out of the chaotic mass of all the stimuli impinging on the organism from the environment, an orderly pattern of information, thus giving rise to a perception of the external situation. But diathesis also operates in the output stage to ensure an orderly response to this perception, in the form of controlled movement adapted to the needs of the external situation.

On Kapp's theory similar considerations apply to all situations in which changes occur in living organisms under the guidance and control of diathesis. In all such changes you have an input selected by the operations of a diathete, which causes a change in the output of the system. The efficient cause of the change in the system is an exchange of energy between the diathetic structure and its environment. But the effect of this energy exchange is determined in part by diathesis. 'One of the causes of a change of state in the controlled element is therefore for every physical change an exchange of energy with the environment. . . . But it is only one of the causes. The other is that the element receives a supply of diathesis in the form of control of the movement when the change occurs. . . . Without the supply of diathesis the movement would have been a random and not a specific one.'

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[1] R. O. Kapp, 'Living and Lifeless Machines', this *Journal*, 1954, 5, 94 n.

[2] R. O. Kapp, *Mind, Life and Body*, London, 1951, p. 124

That is to say, whenever a change occurs in a system, as a result of perturbations due to random energy exchanges with the environment, we cannot expect to find anything but random output, *unless* there has been an intervention in the process by the immaterial agency which Kapp calls a diathete. ' So we are led ', according to Kapp, ' to look for a mechanism that can only exist in a living substance, that has a very specific . . . action.'[1] The words ' that can only exist in a living substance ' sufficiently indicate the vitalistic assumption underlying Kapp's theory of diathesis. The suggestion is that a ' diathete ', an immaterial agency, not subject to the laws of physics, is required to explain any reaction in which random disturbances produce non-random results. It is this contention which I shall now refute by discussing the case of the self-winding watch.

A self-winding watch is essentially a mechanism for converting some of the energy of random movements of a human arm into the controlled movements of the components which go to make up the works of the watch. The self-winding watch achieves this by means of a ' rotor ' mechanism which responds selectively to certain preferred components of the motions of the human arm to which it is strapped (the input) so as to store potential energy in a coiled spring—the mainspring of the watch. This potential energy is then used to move a system of geared wheels, which deliver the output of the machine, viz. the motions of the hour, minute, and second hands. In one sense, of course, the movements of a human arm are not random but controlled; in part by conscious volition, and in part by the various involuntary neural processes which go on in the motor areas of the brain. But this fact is irrelevant in the present context: since the factors which control the movement of a human arm have in general no relationship to, or correlation with, the positions of the hour, minute, and second hands of the watch; and it is the controlled movement of the latter which is the purpose of the watch. In relation to the movement of the hands of a watch the movements of the human arm to which the watch is strapped are random.

A watch comprises two mechanical systems: (i) a *power* system, consisting of the mainspring and the chain of wheels which it drives; (ii) a *control* system, consisting of the escapement which ensures that the energy derived from the power system, through the unwinding of the mainspring, is drawn off at a precisely controlled rate, at specified moments of time. This is achieved through the jewelled escapement lever, machined in the shape of a miniature anchor, the flukes of which engage the teeth of the so-called ' escape ' wheel—the escape wheel being the last of the train of wheels powered by the mainspring which move the hands of the watch. The escapement lever oscillates, under the influence of the hairspring, in such a way as to release one tooth of the escape wheel at a time, alternately stopping and releasing the escape wheel. This causes the train of wheels geared to the power system, the mainspring system, and the watch hands themselves, to move intermittently, in precisely controlled jumps, rather than continuously. Thus a self-winding watch is a complete answer to the question which Kapp asks (when he attempts to establish the peculiar nature of diathesis, and which he considers cannot be answered by any inorganic mechanical system) :

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[1] R. O. Kapp, *Mind, Life and Body*, London, 1951, p. 183

' Is a system possible in which a continuous and random movement of particles causes a specific effect intermittently and only at specified moments of time ? '[1]

Kapp might contend perhaps that the movements of the particles constituting a human arm which winds a self-winding watch, though random, were not ' continuous '. It is of course true that the arm of a human organism, conceived as a macroscopic object, does not execute continuous movements ; and that there are periods of time during which it is at rest in relation to the inertial frame of reference provided by the Earth. During such periods the spring of a self-winding watch will not be wound and it will receive no potential energy. But there are other periods when such a human arm *is* in continuous motion with respect to the centre of gravity of the earth (the origin of the inertial frame of reference), and we may fairly assume that during such periods (which are the ones during which the mainspring of the self-winding watch is wound) the constituent particles of the human arm are also in continuous motion. If this be granted it is clear that the self-winding watch affords a simple example of an inorganic mechanism which has all the essential characteristics which Kapp ascribes to a diathete. Moreover the self-winding watch is a mechanism which is completely subject to the laws of physics. It therefore serves to refute Kapp's theory of diathesis.

However, although the example of the self-winding watch refutes Kapp's theory as it stands, it does serve to draw our attention to certain remarkable features, in certain types of mechanisms, which may provide a clue to a fully mechanistic treatment of living structures. To see this it is necessary to examine significant features of the self-winding watch which make it a ' eudiathetous ' mechanism in Kapp's sense ; in other words, those features which enable the self-winding watch to cause random movements of a human arm to produce specific events at specified moments of time. By doing so we can exhibit peculiar features of eudiathetous mechanisms which are analogous to certain organic processes in living structures. In particular we shall find an analogy between some of these significant features in the self-winding watch and some processes in photosynthesis, which may point the way towards the closer assimilation of certain mechanical and organic processes, thus helping us towards the ultimate goal of exhibiting all living processes as developments of processes already found in non-living mechanisms.

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[1] R. O. Kapp, *Mind, Life and Body,* London, 1951, p. 153

The significant features are as follows :

A Power System comprising, *inter alia*, the following :

(i) *A Random Source of Energy.* In the self-winding watch this source is the motion of the human arm. But in other eudiathetous mechanisms any source will qualify provided its components are *random:* for example the ' white noise ' of solar radiation.

(ii) *A Selective Mechanism which picks out certain preferred components from the random source of energy.* In the self-winding watch this is the ' rotor ' mechanism. This rotor arranges that when the motion of the arm to which the watch is strapped has a component of momentum in the direction of the line joining the figure ' 12 ' to the figure ' 6 ' on the dial, above a certain threshold, this component turns a wheel which winds the spring. The rotor mechanism will not respond to any component in the direction ' 3 ' to ' 9 ' or ' 9 ' to ' 3 '.

(iii) *A means of storing the energy derived from the preferred components selected from the random source of energy.* This is necessary if the random forces, which are the source of energy, are to be conserved, so as to produce ' a specific ' event at specified moments of ' time ' (Kapp's criterion of diathesis). In the case of the self-winding watch the work done by the source, the random movements of the arm, is stored as potential energy in the mainspring of the watch.

(iv) *A ' Cut-out ' mechanism to prevent the overcharging, by the selected components of the random source of energy, above a certain critical level at which the stability of the system is endangered.* Such a cut-out mechanism is provided in the self-winding watch by a release mechanism, which operates when the tension in the spring goes beyond a certain critical value. It ensures that until the tension falls again below the critical level the rotor mechanism will rotate idly, doing no work upon the mainspring. Thus information as to the state of tension in the spring has to be fed back into the cut-out mechanism, and this kind of feed-back is a general feature of all ' cut-out ' devices.

(v) *A control system for the effective delivery of the stored energy to the right place at the right time.* In the self-winding watch the controlled release of the potential energy is arranged by the escapement system described above. The hairspring of the escapement system receives, and stores, the information provided by the movement of the jewelled escapement lever, and converts this information into instructions to the mainspring system through the medium of the escape wheel.

2 *The Self-winding Watch and Photosynthesis*

To conclude this paper I shall mention certain aspects of the process of photosynthesis which exemplify some of the characteristically eudiathetous processes of the self-winding watch which I have mentioned above. It has been usual to represent the total process of photosynthesis by the following simple formula which is probably true to a first approximation :

$$CO_2 \text{ (Carbon Dioxide)} + H_2O \text{ (Water)} + \text{Light} \rightarrow (CH_2O) + O_2.$$

But analysis has shown that two distinct systems are involved, one of which is essentially a *power* system requiring an external source of energy (solar radiation) ; and the other a process of *controlled* chemical synthesis, whereby the chemical energy derived from the power system is used to reduce carbon dioxide to other substances which are synthesised for use by the plant. Thus, photosynthesis, like the self-winding watch, can be seen to involve the two aspects of a *Power* system and a *Control* system, involving

(*a*) the photolysis of water molecules into free molecular oxygen and hydrogen in a combined form—the so-called ' light ' reaction for which energy, derived from components in the visible portion of the spectrum of solar radiation is necessary ;

(*b*) the conversion of carbon dioxide molecules to reduced carbon compounds, out of which other biological processes in the plant cell can synthesise fats and proteins. This is the so-called ' dark ' reaction : since it does not require visible radiant energy for its operation.

Experiment has shown that the ‘ light ’ reaction of photosynthesis —the *power* system—is controlled by the ‘ dark ’ reaction ; in that when radiant energy in the form of light is given continuously the rate of photosynthesis is limited by the rate at which this second reaction proceeds. In this respect, therefore, the first phase of the reaction may be compared to the self-winding mainspring system of a watch whereas the second phase of the reaction may be regarded as analogous to the escapement system of a self-winding watch.

The *Power system* in photosynthesis arranges that certain components are selected from the random source of energy, the ‘ white noise ’ of solar radiation, through the medium of chlorophyll, to supply the energy necessary for the photolysis of water into molecular oxygen and hydrogen in a combined form, and for the reduction of carbon dioxide to carbohydrate and other compounds.

The *Control system* in photosynthesis consists in the living plant cell of the organised system of enzymes which regulate the rate at which the processes go, and ensures that the unstable intermediate products of photosynthesis do not revert before they have played their parts. When sunlight falls upon the chlorophyll molecules contained in the plant’s chloroplasts, these molecules absorb photons from both the violet and red ends of the visible spectrum, and reflect photons from the green-orange region—hence the visible colour of yellow-green which the chloroplasts have. When radiant energy is absorbed by a substance the energy may be released in various ways. The absorbed energy may merely increase the temperature of the substance by increasing the random movements of its molecules about their mean positions, thermal energy depending upon the movement of whole molecules. (Radiant energy absorbed in this way has no significant effect in photosynthesis.) Or the energy absorbed may be used to effect internal changes within the molecules by causing transitions between energy levels. As far as photosynthesis is concerned the significant effect of the absorption of energy is to raise the chlorophyll molecules to ‘ excited ’ energy levels : the molecules which absorb ‘ violet ’ photons being raised to a higher energy level than the molecules which absorb ‘ red ’ photons. Molecules which are lifted to an excited energy level by the absorption of radiant energy as a general rule slip back to their ground state with the release of the same amount of radiant energy as they have previously absorbed by the process known as ‘ fluorescence ’. When, however, these molecules are subject to a steady bombardment of photons of radiant energy, i.e.

when light is given continuously, they will be re-excited again as soon as they have released energy: so that they will be kept in a state of practically continuous excitement.

A peculiarity of the chlorophyll molecule is that, while it absorbs radiant energy strongly in the violet region of the visible spectrum, it releases energy by fluorescence only in the red region of the visible spectrum. This fact is remarkable because the absorption of a violet photon raises the chlorophyll molecule to a higher level than the red-photon absorption level. The explanation according to the current quantum theory, is that the energy difference is accounted for by a ' radiationless ' transition, within the molecule, in the course of which certain constituents of the molecule receive kinetic energy, which is ' free energy ' in the thermodynamic sense. Thus the chlorophyll molecule may be regarded as a chemical ' machine ' for the conversion of radiant energy into free mechanical energy. It is this conversion, of absorbed radiant energy to ' Gibbs free ' kinetic energy within the molecule, which provides the necessary power to do the work of breaking the valence bond between hydrogen and oxygen in the water molecule. Thus, unlike other processes in which energy absorbed from sunlight by a coloured compound is dissipated subsequently in increased thermal energy, or through fluorescence, or through the chemical degradation of the substance (as when sunlight causes the fading of coloured dyes), the absorption of the components of sunlight by chlorophyll provides a source of ' free energy ' in the thermodynamic sense; which free energy is used to reduce a chemically inert substance to highly reactive constituents.

The action of chlorophyll, in selecting certain preferred components from the random ' white noise ' of solar radiation, and in utilising these components to yield ' free energy ', is clearly analogous to the action of the selective ' rotor ' mechanism of the self-winding watch, that utilises selectively certain preferred components of the random motions of the human arm, to provide the free energy required to drive the watch. Moreover, when the photo-chemical process of splitting water into its constituents, hydrogen and oxygen, is slowed down, or prevented, there is an increase of fluorescence: the chlorophyll then gets rid of more of the energy absorbed from violet components (part of which is converted to useful kinetic energy by the 'radiationless transition '), through increased fluorescence in the red emission band. This connection, between variation in the rate of photolysis of water and rate of fluorescence of the illuminated chlorophyll molecule, implies a rather sensitive feed-back or ' cut-out ' mechanism, although fluorescence never disposes of more than a very small percentage of the radiant energy absorbed by the chlorophyll. It is clear, however, that some mechanism, other than increased fluorescence, must operate to dispose of surplus free energy under conditions of high illumination and low photochemical activity, so as to protect the plant cells from photodynamical destruction. One suggestion is that the plant cell in such conditions used part of its surplus free energy to convert carotenoids to phytol chains, and to attach these phytol chains to magnesium porphyrin rings, thus synthesising chlorophyll molecules, in replacement of those chlorophyll molecules photodynamically destroyed when the chlorophyll machine is, so to speak, ' running light ' (i.e. when the production of free energy is in excess of the requirements of the water photolysis). On this hypothesis surplus free energy generated by the radiationless transitions of the chlorophyll molecules, activated by the ' white noise ' of solar radiation, would be accounted for (1) by photodynamical destruction of some chlorophyll molecules, and (2) by synthesis of new chlorophyll molecules from the carotenoids and porphyrins present in the plant cell, these two processes being in dynamical equilibrium with respect to the prevailing conditions of illumination and photolysis of water. Such dynamic equilibrium would constitute a ' cut-out ' mechanism sufficient to protect the stability of the plant cell. Of course the existence of this suggested cut-out mechanism is a speculation suggested by the self-winding watch analogy set out above. But the hypothesis, though speculative at present, is clearly susceptible of experimental verification or disproof. The acceptance of the hypothesis (in consequence of favourable experimental evidence) would entail some revision of current ideas, about the rôle of the chlorophyll molecule in photosynthesis, according to which the chlorophyll molecule is regarded as a *catalyst.* According to the hypothesis sketched above, on the basis of the analogy to the self-winding watch, the chlorophyll molecule functions as a chemical *machine* (rather than a catalyst) for the generation of free energy from sunlight ; and, therefore, like other machines is liable to suffer damage (even destruction) if it ' runs light '.

We may sum up this description of the operations of the ' power-system ' of photosynthesis by saying that its result is the splitting of water, a substance of low potential chemical energy, into two highly reactive constituents, oxygen and hydrogen, the energy necessary having been derived from the selection of certain preferred components from the ‘ white noise ’ of solar radiation. The hydrogen formed in this way is not liberated in free molecular form (as is the oxygen), but is combined with some substance which has not yet been identified chemically, so as to constitute a hydrogen donor of high potential chemical energy. The sensitive feed-back relation, between the rate of photolysis of water and rate of fluorescence in the chlorophyll, suggests that the unidentified hydrogen donor may be chlorophyll itself, or a chlorophyll complex. This at present unidentified hydrogen donor substance so to speak ‘ traps ’, temporarily, the hydrogen liberated from the oxygen by the photolysis of water, and stores it in a readily available form for the reduction of carbon dioxide. It also appears to function as a ‘ cut-out ’ mechanism with respect to the power-system in that, as we have noted above, the rate at which the photolysis of water proceeds, in relation to the quantity of radiant energy absorbed by the chloroplasts, is determined by the rate of the ‘ dark reaction ’ by means of which carbon dioxide is reduced by acceptance of hydrogen from the hydrogen donor.

Thus the power-system in photosynthesis, provided by the action of the chlorophyll molecule, has the four features we have noted above in connection with the power-system of the self-winding watch. These features can be tabulated in the following way :

(1) *Random source of energy*	Random motion of the human arm	White noise of solar radiation
(2) *A selective operator to detect and convert certain components from the source of energy to Gibbs free energy*	The ‘ rotor ’ of the self-winding watch	The absorption spectrum, and the ‘ radiationless ’ transitions, of the chlorophyll molecule
(3) *Storage of the energy derived from the selected components of the random source of energy*	The mainspring system of the self-winding watch	The unidentified compound which traps the hydrogen, freed by the photolysis of water
(4) *Cut-out mechanism to avoid oversupplying the storage system*	The cut-out release which prevents overwinding of the mainspring of the watch	The reaction of the hydrogen-trap upon the unknown mechanism for disposing of excess excitation energy not required for the photolysis of water

So much for the analogy between the *power-systems* of the self-winding watch and photosynthesis. As regards the *control-systems* there is little that can usefully be said at present (apart from what I have already said in connection with the ' cut-out ' mechanism of the power-system), because of our ignorance of those processes of organisation within living cells that control the balance and rate of enzyme activity. Moreover, up to the present no one has succeeded in achieving a *complete* photosynthetic process *in vitro* without a living plant. This suggests that there are elements in the process which depend upon the organisation of living matter.

It may then be asked : What is the value of this analogy between the self-winding watch and the phases of photosynthesis which have been identified in the laboratory ? The answer is that it is of some heuristic value to point out analogies between the structure and functions of non-living mechanisms and living organisms wherever these analogies can be established. For it is only by the resolute extension of mechanistic thinking to its utmost limits that we can hope to avoid a premature and unnecessary form of vitalism of which I believe Kapp's theory of diathesis to be one example. (Professor Eccles' ' interactionist ' theory, expounded in his *Neurophysiology of Mind*, may be another example.)

Ministry for Internal Defence and Security
Kuala Lumpur

NOTES AND COMMENTS

Irreversibility; or, Entropy since 1905

1 To avoid misunderstandings which might arise otherwise, I wish to make clear at the outset that *I do not intend, in this note, to suggest that the second law of thermodynamics is in fact false.* What I wish to suggest is that the logical situation in connection with the *entropy law* (as I shall here call the 'second law of thermodynamics') is highly unsatisfactory, and that it has been so ever since 1905.

That the law is in need of a kind of phenomenological formulation had been realised long before that date; and Planck formulated it in analogy to the following formulation of the '*first law*' which, as is well known, can be formulated as the '*law of the excluded perpetual motion machine of the first order*':

> (I) There does not exist a perpetual motion machine of the first order; that is to say, an otherwise closed physical system which either constantly pumps heat into its physical environment, or which constantly absorbs heat from its physical environment.

The entropy law (second law) may be formulated by analogy, as follows:

> (II) There does not exist a perpetual motion machine, of the second order, that is to say, a physical system, immersed in a heat bath, which, by cooling down (or, which is the same, by absorbing heat from the surrounding heat bath), can move a heavy body against a force, thus increasing its potential energy; or in *terrestrial* terms, a machine which, by cooling down, can *lift a weight.*

I am not aware that a better phenomenological formulation of the entropy law exists.

The *explanation* of this law in terms of the kinetic theory of gases is well known. It should be observed that this was developed by Maxwell and Boltzmann at a time when molecules and atoms were considered either as *mere instruments of thought* (useful fictions, mathematical hypotheses); or else as real, but as so small that we would never obtain direct experimental evidence of their individual existence.

Accordingly, few physicists were shocked by an unintended result of the kinetic or statistical 'explanation' of the entropy law: I am alluding to the result that the entropy law is not strictly true, but holds in the overwhelming majority of cases. It was felt to be quite satisfactory that no deviations from the law were expected to be *observable*, according to the statistical theory; for in this way, the *phenomenological* law would be fully explained.

2 The purpose of this paper is to draw attention to the fact that this attitude became untenable the moment it was accepted that deviations from the law can be in fact observed. This was accepted in 1905, following Einstein's first papers on the Brownian movement.[1]

According to Einstein's theory we are forced to say that in the Brownian movement, observable heavy particles are sometimes lifted against the gravitational field of the earth, at the expense of a (slight) cooling down of the liquid. Of course, just as many particles are sinking, thus restoring the lost heat. But this does not matter: the letter of Planck's law is undoubtedly violated, as Einstein in fact stated.

This situation is a little more serious than is usually seen. For the now generally accepted theory of the Brownian movement clearly implies that, precisely as small fluctuations affect small suspended bodies, big fluctuations will affect big suspended bodies; and it implies, moreover, that big fluctuations, although very improbable and therefore very rare, must occur, precisely as small fluctuations must occur.

All this is of course very well known to every physicist; and I believe that most physicists will accordingly agree that the entropy law, in Planck's formulation, is simply falsified by the Brownian movement, as interpreted by Einstein. However, they may say, perhaps, that the Maxwell-Boltzmann law is certainly not falsified but supported by Einstein. This is quite correct. But the following consequences have not, apparently, been drawn from the situation which I have described.

(i) If we give up Planck's law of the excluded perpetual motion machine of second order, without restating it in some form, then there is nothing left in our physical laws to tell us that we cannot build such a perpetual motion machine. (The appeal to the exclusion of Maxwell's demon does not help; for the impossibility of the demon is derived from the fact that otherwise the *phenomenological* entropy law—in a form similar to Planck's—would become untrue.)

(ii) If we wish to retain some form of the principle that a perpetual motion machine of the second order cannot be built, then we have to reformulate a phenomenological law and to prove that the formulation is consistent with the present interpretation of the Brownian movement.

3 The present situation is logically highly unsatisfactory; for in many arguments within statistical mechanics (and also in information theory), use is made of the fact that certain events are impossible because otherwise the entropy law (in its phenomenological or Planckian form) would be violated. Clearly, all these arguments are circular.

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[1] Einstein, *Annalen der Physik*, 1905, **17**, 549-560; **19**, 371-381

The fact that these arguments are employed illustrates my claim that, although nothing new has been said here, physicists habitually fail in this point to put two and two together. Moreover, they fail in this point not accidentally, but because no satisfactory phenomenological formulation of the entropy law exists. It seems to me that the difficulties in the way of a satisfactory formulation are formidable.

One reformation which suggests itself would be to insert the word 'constantly' (or 'as often repeated as we like') into Planck's formulation of the entropy law. But this does not seem satisfactory since, according to Boltzmann's theory, fluctuations are of course repetitive; that is to say, they occur again and again. Admittedly, they are rare. But their rarity hardly justifies us in asserting that *usable fluctuations are too rare to be technically utilised.* And although this last formulation may express our intuitions and our intentions quite well, the terms used are much too vague, and they suggest that we know in advance what kind of fluctuations can ever be technically utilised. (Moreover, unicellular organisms may utilise the Brownian movement.)

A satisfactory phenomenological formulation of the entropy law, and one which appears to be in full agreement with our intentions appears to be in the following:

> (F) A gas or a liquid in a closed circular tube, immersed in a heat bath of any temperature and fitted with a *one-way valve*, does not constantly circulate through the tube, however slowly.

An equivalent formulation seems to be:

> (F′) There does not exist a semi-permeable membrane with an asymmetric structure (like a one-way valve) such that the probabilities of passing through it are not equal in both directions.

This formulation is logically very much weaker than the one alluded to by von Neumann,[1] where we read: 'Our arrangement is rather similar to "Maxwell's demon", i.e. to a semi-permeable wall which transmits molecules coming from the right and reflects those coming from the left.' For von Neumann's wall is supposed by him to work with the transition probabilities 1 and 0, while all I propose to demand is that the probabilities are unequal.[2]

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[1] J. von Neumann, *Mathematical Foundations of Quantum Mechanics,* translated by R. T. Beyer, 1955, p. 369

[2] In the same place in von Neumann's book, we find the following unsatisfactory argument against his wall: '. . . and therefore, by the second law of thermodynamics, such a wall cannot exist.' The argument is unsatisfactory because the 'second law' here used as an independent authority is Planck's version—*refuted* since 1905—to which von Neumann refers in note 184 on p. 359. The argument is thus much worse than if it were merely circular (which it also is).

4 I think that everybody will agree that either of these formulations (F) or (F′), if not equivalent to the intended entropy law (as I think it is) must at any rate be derivable from it. (My formulations are intended as a phenomenological version of the assertion that even a probabilistic, or fallible, Maxwell demon does not exist.) Accordingly, any theory which purports to explain the entropy law would have to be strong enough to entail the formulae (F) and (F′). I have little doubt that my formulae (F) and (F′) cannot be derived from any of the versions of statistical mechanics, as they exist at present, except of course if the non-existence of a Maxwell demon is simply assumed *ad hoc*, in some form or other. And it will be extremely difficult—I fear even impossible—to prove that my formulae (F) and (F′) are consistent with the present theory of Brownian movement. My reason for holding this pessimistic view is this. In the present theory,[1] the absolute size of the 'molecules' does not play any rôle, and gases consisting of molecules of 'a very large mass'[2] are freely used in the discussions. But we know that for sufficiently big 'molecules', we can and do in fact build the forbidden valves and membranes. (These of course consume energy, but this does not matter since it is replaced by the heat bath.) Thus the logical situation is highly unsatisfactory. This has led to attempts to rescue it by dragging in the subjective probability theory, i.e. states of our knowledge. But neither Planck's formulation nor those suggested here can ever be validly derived in this way.[3]

5 A typical example of a discussion of these problems which from the point of view here outlined is completely unsatisfactory is Szillard's discussion of Maxwell's demon (cf. footnote 4, above).

(*a*) He first gives four non-equivalent formulations of the entropy law clearly implying that they are all equivalent. His *first* formulation which operates with the concept of a 'lowest temperature' is hardly clear enough and hardly phenomenological; moreover it demands *constant* or *persistent* output of work, in contradistinction to his *second* formulation which is practically the same as is Planck's. His *third* formulation is very interesting, because it is an attempt to incorporate Einstein's results of 1905; but for this very reason it contradicts the second. Also, it is not phenomenological. It runs as follows: 'If we attempt to use fluctuations in order to extract energy at the expense of heat, then we embark upon a game of chance in which the expectation of a gain is either equal to zero or less than zero.' Thus we may gain—and refute Planck—even though we shall later probably lose our gain (and more). The *fourth* formulation simply does not touch the problem.

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[1] See, for example, von Neumann, op. cit., pp. 361 sqq.; A. Einstein, *Verhandlungen d. deutschen physikalischen Gesellschaft*, 1914, **12**, pp. 840 sqq.; L. Szillard, *Zeitschrift fur Physik*, 1925, **32**, pp. 753 sqq., and 1929, **53**, pp. 840 sqq.

[2] Following Einstein, von Neumann, op. cit., uses gases consisting of 'molecules' which are boxes each of which 'must have a very large mass'.

[3] For a criticism of the subjective theory of probability, see the *Postscript* to my *Logic of Scientific Discovery*, 1957.

(*b*) Szilard then assumes the axiomatic validity of the entropy law so defined.

(*c*) He then proves that a Maxwell demon (which he takes to be an intelligent being rather than a valve, or an asymmetric membrane) would have to pay for his information concerning the oncoming molecules by an increase of entropy ; which is a trivial consequence if *any form* of the entropy law is to be preserved intact ; and which therefore follows if the entropy law is assumed as a premiss.

(*d*) He thereby shows that information or knowledge can be measured in terms of negative entropy, or entropy in terms of negative knowledge ; which immediately leads to the interpretation of statistical probability in terms of subjective ignorance.

But I think that hot air would continue to escape entropically even if there were no intelligent beings about to provide the corresponding amount of nescience.

KARL R. POPPER

A Note on Multi-Dimensional Time *

H. A. C. DOBBS, in a note contributed to this *Journal*,[1] has hinted at a theory in which time figures as a complex variable with an imaginary component. I have been enquiring into the topological requirements of hypotheses about multi-dimensional time. A brief statement of some of my results may be of interest to the readers of this *Journal*. To demand a multi-dimensional time is to conceive, in the case of time, something analogous to the extension of metrical geometry to the Euclidean E^n-space, the infinitely many-dimensional Hilbert space, E^ω, etc. It is not difficult to exhibit the set of points in Euclidean spaces of two (or more) dimensions as a case of a partially ordered set, since the ordering relation is given by

$$(a, b) \leqslant (c, d) \text{ if } a \leqslant c\,;\; b \leqslant d.$$

Partial ordering in topology is a generalisation of the familiar linear or total ordering. Various forms of a multi-dimensional theory of time can be extracted from Dunne's well-known hypothesis[2] if this is regarded not as a variant of Hinton's ' thread-filled ' model of the universe, or as a ' regressive method ' of analysing ' transitoriness ', but rather as the postulation of ' extra dimensions ' of *time*. Broad [1] dismissed Dunne's ' infinity of dimensions ' as a superfluity foisted on us by the ' regressive method ' of analysing ' becoming ' (i.e. we want a time T_2 to time the ' travelling ' along T_1 and so on). We may discard the ' regressive method ' of analysis and yet retain Dunne's ' infinity of dimensions ' as the demand for *very general topological representations of a multi-dimensional time.*

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* I have to thank the Syndicate of the Madras University for permission to use material in my doctoral dissertation entitled ' On Some Spatial Representations of Time '.

[1] H. A. C. Dobbs, ' The Time of Psychology and of Physics ', this *Journal*, 1953, 4, 162

[2] J. W. Dunne, *An Experiment with Time*, London, 1939

Consider a set U in which a topology has been defined by Hausdorff's axioms for ' neighbourhood ' (i.e. a Hausdorff space). This set is to satisfy the following conditions : (1) There is a linear subset L which is the topological image (homeomorphic mapping) of the set of real numbers. The elements or points of the subset have the same order-type as the real numbers, i.e. the ordering relations of the line can be transferred to the points of L ; (2) Two different points A and B on L are taken so that the points between A and B correspond to the set of real numbers between two real numbers. The subset on L so obtained may be denoted $L(A, B)$; let us define now a class of subsets Σ of U such that (i) each of the subsets Σ shall intersect L in the subset $L(A, B)$, or in the usual topological notation, $L(A, B) = L \cap \Sigma$; (ii) the subsets Σ are *totally* or *linearly* ordered, i.e. between any two of them there is a dyadic, asymmetrical, transitive relation which can be formally represented by the relation of $>$ or $<$ holding between the real numbers,[2]

In this spatial representation of a multi-dimensional time, conceived along the lines of Dunne's *An Experiment with Time*, $L(A, B)$ is the familiar ' linear time ' between two chosen instants of ' clock-time ', say between the instants of ' birth ' and ' death ' of an individual. The class of subsets denoted by Σ corresponds to the ' epochs ' (if we may so call them) of multi-dimensional time. The entire representation reduces to a two-dimensional time if U is the plane with the usual topology, L is the x-axis (any line can be taken as the x-axis), and $L(A, B)$ is the segment, $B > A$, on L. The requirement (1) specified above is satisfied. The class of subsets Σ may be pictured as the system of rectangles :

$$\Sigma_\alpha : \ A \leqslant x \leqslant B, -\alpha \leqslant y \leqslant \alpha$$

where $0 < \alpha < \infty$. The requirements 2(i) and 2(ii) specified above are satisfied since $\Sigma_\alpha \subset \Sigma_\beta$ if $\beta > \alpha > 0$.

A more general representation can be worked out along the following lines. Let R denote the straight line $(-\infty, \infty)$. Let N be any cardinal number and let R^N be the topological product of R taken N times. Let

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[1] C. D. Broad, ' Mr Dunne's Theory of Time in *An Experiment with Time*, *Philosophy*, 1935, **10**, 168

[2] ' Set-inclusion ', as usually defined in topology, is dyadic, asymmetrical, transitive, and also reflexive.

$L(A, B)$ be the straight line segment, $A \leqslant x \leqslant B$. Let Σ_α, $0 \leqslant \alpha < \infty$, be the set in R^N specified by the Cartesian product

$$L(A, B) \times I_\alpha^{N-1},\ I_\alpha = -\alpha \leqslant x \leqslant \alpha.$$[1]

A hypothesis analogous to that hinted at by Dobbs can be formulated as a special case of the general topological representation I have sketched. Suppose each of the points or elements in the subsets Σ postulated above can be represented by a complex number of the form $a + ib$ where $i = \sqrt{-1}$. We may introduce a 'clock-time' t and a 'subliminal time' τ as subsets of suitably chosen Σ. 'Clock-time' t, for instance, may be viewed as a single-valued, increasing function of the 'ultimate time' Σ, i.e. the real number $t(\Sigma_2)$ may be viewed as greater than the real number $t(\Sigma_1)$, if $\Sigma_2 > \Sigma_1$. An instant of 'subliminal time' may be represented by an equation of the type

$$\tau = \phi(\Sigma) + i\psi(\Sigma)$$

where $i = \sqrt{-1}$ and ϕ and ψ are single-valued and continuous but not necessarily increasing functions of the 'ultimate time' Σ. Suppose the imaginary part $i\psi(\Sigma)$ in the equation of 'subliminal time' vanishes or becomes zero for a particular value, say Σ_1 of Σ. Suppose $t(\Sigma_1) = t_1$ and $\tau(\Sigma_1) = \phi(\Sigma_1) = t_2$ where $t_2 > t_1$, there can be, *ex hypothesi*, *two values* of t corresponding to an 'epoch' of the 'ultimate time' Σ. I suggest that these would be analogous to the 'present' and the 'quasi-future' in the hypothesis contemplated by Dobbs.

Mundle, in this *Journal*,[2] has raised crucial questions about the *interpretation* of a formally conceived multi-dimensional time if the 'extra dimensions' are additions merely in the 'extensional' aspect of time. It should be obvious that 'extra dimensions' of time cannot be introduced without our envisaging the most radical transformation of our notions *both* about the 'extensional' and the 'transitory' aspects of time. There are no grounds for supposing that the newer ideas would be intuitively evident. The extreme uncertainties about the interpretation of the symbol t in certain newer quantum-mechanical theories should warn us that we may have to contemplate departures from familiar notions of time-ordering even if we cannot give concrete interpretations of all the symbols. The Feynman 'model'[3] of the universe introduces a positron as a particle whose time-co-ordinate *decreases*. And if we introduce a S-matrix (following Heisenberg) and an associated 'fundamental length', L_0, it remains to be seen whether the term ' wave-function at a given point of space-time ' can be unambiguously retained in an adequate future quantum-mechanical theory.[1] At a recent International Conference at St Paul de Vence, France, Pascual Jordan discussed the new trends in physics and said that the hypothesis of a multi-dimensional time is worth careful study even if it turns out in the end to be an oversimplified description of the universe.

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[1] Dr V. G. Iyer, Professor of Mathematics at the Annamalai University, South India, has pointed out to me that no Σ_α can be given the form of a metric if N is $>$ aleph$_0$ $(=\aleph_0)$.

[2] C. W. K. Mundle, 'Mr Dobbs' Two-Dimensional Theory of Time', this *Journal*, 1954, **4**, 331-337

[3] *Rev. Modern Phys.*, 1948, **20**, 367 ; *Phys. Rev.*, 1949, **76**, 769

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[1] See, for instance, E. M. Corson, *Perturbation Methods in the Quantum Mechanics of n-electron Systems* (London, 1951, ch. xii)

DISCUSSION

MR DOBBS ON 'DIATHESIS, THE SELF-WINDING WATCH, AND PHOTOSYNTHESIS'

MR DOBBS' apparent disagreement with my philosophy seems to be due to a misapprehension of what it is. He thinks that I make a basic distinction between living and lifeless machines and says so in his first sentence. This is incorrect. I have insisted on the resemblance of both and have advocated the word ' diatheme ' for them. By contrast I describe what is neither a living nor a lifeless machine as an adiathetous structure.

What living and lifeless machines have in common and what distinguishes them from adiathetous structures is that they show specific order and their component parts serve a purpose, be it only that of ensuring that the whole machine shall work in a specific way. The lifeless machine, be it an electronic computer, a toy tortoise, or a self-winding watch, only does what it has been designed to do if another machine, a living one in every case, does something specific to it. The owner of the watch must, for instance, ensure that it be properly shaken on occasion. The watch does not really wind itself. Diathesis is applied to the watch for its design, for its construction, and for its operation. It is emphatically *not* the result of random events.

It is true enough that some of the things that are done to the watch are random. The logical mistake is to jump to the conclusion that all the things are. For the watch to be wound up there must be selection of the minimum intervals between the shaking, of the nature of the shaking, and of its minimum duration. Here the watch does not differ from other machines, living or lifeless. The movement of the molecules of steam that enter the cylinder of a steam engine is random, but this does not prove that the steam engine can work without an operator.

The analogy drawn by Mr Dobbs between the operation of a self-winding watch and that of a leaf in process of photosynthesis is quite a good one and can be drawn between any living and any lifeless machine, but the conclusion that Mr Dobbs reaches is strangely illogical. The watch has been invented and designed, a specification has been prepared for it and this has been handed to the watchmaker, who has followed the specification carefully. The watchmaker's activities were certainly not random. The purchaser of the watch has carried out the instruction that he must wear it on his arm for not less than so many hours a day. Now and then, though at long intervals, he adjusts it. Thus, like a steam engine or an electronic computer, the self-winding watch is the consequence of care, selection, discrimination, control, of what I call diathesis. A green leaf has much in common with a self-winding watch, says Mr Dobbs, and I agree with him, but I do not agree with Mr Dobbs nor any of the others of the same now-fashionable school about the conclusion that they derive from the resemblance. With sufficient ingenuity one can, they say rightly, contrive a device in which some random events lead to an ordered result, provided one makes the device carefully and properly controls its operation. This proves, they continue with perverted logic, that random events can lead to an ordered result in a system that has not been devised, on the construction of which no care has been exercised, and that is not being subjected to any control.

The philosophy dear to the hearts of many in these days is that green leaves, human brains, and other living machines are *not* the result of selection, guidance, control, discrimination or any other diathetous activity. Perhaps they will some day find an argument in support of their contention. But they cannot find support from the resemblance of the living machine to one that *is* the result of a diathetous activity.

Reginald O. Kapp

THE PHILOSOPHY OF SCIENCE GROUP

Nottingham Conference Programme

Friday, 20th Sept. Arrive for dinner.
Evening : ' The Logic of Probability '
Speakers : Dr J. Bronowski and Dr. W. Mays
Chairman : Mr W. C. Kneale

Saturday, 21st Sept.
Morning : ' Observation and Experiment '
Speakers : Mr P. Alexander, Dr Mary Hesse, and Mr R. Withers
Chairman : Professor H. Dingle
Evening : ' Homeostasis '
Speakers : Professor C. A. Mace and Dr N. Walker
Chairman : Professor R. O. Kapp

Sunday, 22nd Sept.
Morning : ' The Mind-Body Problem '
Speakers : Professor J. H. Woodger and Dr C. K. Grant
Chairman : Dr E. H. Hutten
Afternoon : General discussion
Depart after tea

REVIEWS

A CYBERNETIC INTERPRETATION OF ECONOMICS

One of the most exciting features of the rapid advance that is being made today in science and technology is the coming together of fields once considered to have little in common. Chemistry and physics have long been close partners; mathematics was applied to 'unscientific' matters of strategy during the war, and is now slowly forcing its way into similar peacetime problems of planning and administration; physiology and neurology have learnt from engineering and electronics, both in studies of control systems in the animal, and in fascinating hypotheses concerning the functioning of the brain; and electronic computers are invading the world of business administration. This book[1] has made an early and important contribution to the conjunction of two most unlikely partners, the exact science of electrical engineering and the 'social science' of economics.

At a time when the shortage of scientists and technologists, particularly engineers, has become so acute that it is at last officially recognised, the protagonist of the arts is liable to fall back on his last defence, and say that if we must produce more engineers, let us at least give them a more liberal education. The reviewer, an engineer himself, has to admit that there is a lot in this view. It is therefore a relief to the ego of the rude engineer to find a book which has been written by an engineer with the express purpose of broadening the education of economists. It has had the unintended effect of broadening the education of at least one engineer.

The basic concept behind this union is that of 'feedback'. This is the jargon used to describe the means by which a control system knows whether it is right or wrong. Such a system is envisaged as one having an 'input' and an 'output', and its object is to make the output follow, or reproduce, the input. A causal chain joins input to output in a conventionally forward direction, and a measurement of the output is signalled back to the input end of this chain along a 'feedback path', a comparison of input and output being used to control the output in the appropriate way via the forward chain. Such systems have been known for a great many years, though not described in these terms; examples are the wind-driven motor which turns a windmill to face the wind, and the James Watt engine governor. During the last war the principle was applied very widely in engineering systems. It was then that feedback systems, whatever their physical nature, were found to exhibit, under certain conditions, very peculiar and undesirable forms of behaviour. They became unstable and broke into oscillation or hunting, so that the output, instead of following the input, surged violently from upper to lower limit of its capability. The higher the performance that the system was designed to have, and the more complex the situation to which it was applied, the more it was liable to behave in this way. A strong theoretical attack was made on this problem, initially with reference to the design of electronic amplifiers, and out of this a new branch of applied science has emerged, often known as systems engineering.

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[1] Arnold Tustin, *The Mechanism of Economic Systems,* William Heinemann, London, 1954. Pp. vi + 161. 25s.

The basic principles of feedback systems are now very well understood ; why they are in a special category, why they become unstable, how to prevent it, how to analyse their performance, and to know the physical limits which it may attain. Moreover a host of design techniques has been produced, which make it possible to design satisfactory systems without the tedious calculations of complete analysis. The subject may be said to be complete as regards a very wide class of systems which can be treated as ' linear '. A linear system is one in which the effect of two causes acting simultaneously is the sum of the effects of each cause acting independently. Many systems, called non-linear, cannot be treated satisfactorily or completely in this way, and for them theoretical knowledge, whilst extensive, is far from complete, and they present many difficulties in analysis and design.

The concept of a control system as one having an input and an output with a feedback path, whilst always valid, is not one that appears superficially applicable in many cases, as for instance that of the windmill. A more universally acceptable concept is that of the ' closed loop ' formed by the forward causal chain and the feedback path. This closed-loop may be seen as a closed chain of cause and effect. For instance in a simple room thermostat system, the temperature of the room affects the thermostat, which by switching heat on or off affects the temperature of the room. Seen in this light it is apparent that many systems not conceived as control systems exhibit closed-loop characteristics ; for instance the way in which an aircraft is flying affects the aerodynamic forces on its wings and control surfaces, and these affect its attitude and so the way in which it flies. The fact that an aircraft, if not properly designed, may be unstable in flight is significant in this context, and in general any closed-loop system has the inherent possibility of instability, and any system which is unstable, or indulges in self-excited oscillations, must be a closed-loop system.

There is a clear analogy with an economic system, which is seen to consist of a large number of interrelated closed loops. The production-consumption sequence is one example ; demand for goods leads to their production, which results in the producers receiving money, and so, in their capacity as consumers, demanding more goods. Economists, at least since Keynes, have been well aware of this, but they have lacked the engineers' general concept and theory of closed-loop systems. One result has been that a postulated closed-loop model, proved to be unstable (that is to result in booms and slumps) has been put forward as *the* explanation of economic instability. Yet any systems engineer could postulate an infinity of different systems, each equally valid as an economic model under present knowledge, every one of which would be unstable. This may be demonstrated very rapidly on a type of electronic computer known as a ' simulator '. This was done recently for the benefit of a person of great responsibility in the practical economic world, and it was shown how a stable system could become unstable if an apparently innocent parameter were changed. His reaction was one of surprise that we have so far managed our economic affairs as well as we have.

The central core of each such model is formed, following Keynes and later economists, by dividing products into consumption goods and investment goods, postulating a different type of closed-loop for each, and then uniting the loops to study the effects of their interaction. The simplest model, consisting only of this, may be elaborated, by analogy with the real economic system, to any degree which may be considered useful. Professor Tustin's book expounds this viewpoint, beginning with the simplest Keynesian model and progressing to a model sufficiently elaborate to serve practical needs many years ahead. He describes the principles of the theory of closed-loop systems and discusses in considerable detail many of the engineers' mathematical tools for their analysis and design, all with reference to their application to economics.

There is no doubt that the author has made a completely convincing case. An economic system is a closed-loop system, and as such it can be, and indeed must be, studied in relation to the general theory of such systems. Moreover there is great hope that in this way we may be able to achieve a means of economic control which maintains overall stability whilst still allowing freedom in detail, freedom which is necessary both for the satisfaction of individual wants and for the attainment of highest efficiency. The author has made a tremendously valuable contribution to this end.

How far does this first step take us ? It is no disparagement to the author, who indeed is well aware of the fact, to reply ' not very far '. The engineer is certainly not in a position to solve every economic problem by application of his magic techniques. For the economic field bristles with problems, even those of a purely scientific or mathematical nature, which the engineer seldom encounters in his own study of control systems. First there is the vastly greater size of the economic problem ; studies of this type must be made on a macro scale, and for many purposes even the limits of national boundaries are too restrictive. Then there are the difficulties of measurement or the gathering of statistics ; to measure on anything like the scale considered necessary in engineering systems would be prohibitively expensive, and one has to fall back largely on making the best use of statistics which have been gathered for other purposes. There are special mathematical problems in the application of statistical methods of estimation to closed-loop systems and there are the well known difficulties involved in the use of short time-series to estimate parameters which may themselves change in a comparable time. The techniques of analysis of linear systems have only limited application to economic affairs, and the engineers' problems of analysis of non-linear systems are present here in greater degree. Finally, whilst close limits can be set to the disturbances on an engineering system, economic disturbances may, as recent events have demonstrated, be of any nature or magnitude.

These difficulties are well understood by the author (the problems of estimation are discussed in a special appendix to the second edition), and he puts forward his own ideas and the general techniques of engineers with great modesty and diffidence, 'in the hope that in some way the rapid progress . . . in the world of engineering may contribute to the solution of the economic problems'. The book in fact suggests many ways in which it may, and one chapter is devoted to the possible use of physical analogues ; here one may make a minor criticism—this is not the best chapter of the book, and it completely neglects the use of digital computers, which are certain to be of enormous value in this work.

This, then, is the present state of the subject. The principle of the economic system as a closed-loop system has been demonstrated. It is possible to postulate economic models of any desired complexity, to analyse these by means of modern computing machinery, and to show that they behave in a broadly similar manner to the real system. Before such models can be of use for quantitative understanding, leading to the design of adequate methods of control, a long and difficult process of measurement and estimation is necessary. After surmounting all the difficulties of this process, the 'control economist' will have one great advantage over the control engineer ; his methods of control will not be subject to the limitations of physical equipment, and he will have all the time in the world, judged by electronic computing standards, in which to make his day-by-day control decisions. Let us hope that this advantage will not be offset by political considerations, from which the engineer is mercifully free.

R. H. TIZARD

Problems of Analysis. By Max Black.
Routledge and Kegan Paul, London 1954. Pp. xi + 304. 21s.

OF this collection of fourteen essays, half have already been published, the other half are new. The topics covered include, among others, scientific method, definition, Zeno's paradoxes, induction, and mathematical logic.

In his essay on ' The Definition of Scientific Method', Black maintains that we need to abandon the search for a timeless and immutable essence *Science*. Neither measurement, nor observation, nor the use of instruments, nor all of them together, can be regarded as essential. He proposes to take seriously the historical aspects of science and especially ' those procedures which, as a matter of historical fact, have proved most fruitful in the acquisition of systematic and comprehensive knowledge ' (p. 13) which seems to be another way of saying that science is what scientists have been doing. However, though history is not ' bunk ' it is not always an infallible guide. Black would seem to be searching for some general scientific attitude exemplified in the history of scientific achievement.

Black points out that the type of definition that consists in giving a connotation of a term in the form of a condition determining a class, applies largely to mathematical and logical notions. If we consider instances of the application of terms from biology, law and politics, and even philosophy, we shall find ranges not classes, since the specimens to which the terms apply are arranged according to their variation from certain typical or clear cases. However, this ' range variation ' would seem often to be the mark of knowledge at an imperfect stage and need not be intrinsic to the instances themselves. Black applies the notion of a range to explain the diverse meanings a self-identical expression may have in different contexts of utterance, which, he suggests, can be organised in terms of their variation from such central cases or paradigms. I doubt myself whether this linguistic relativity is as general as philosophers sometimes make it out to be. If, despite changes in circumstance, our expressions had not a fairly constant meaning, communication between ordinary persons would be much more difficult than it really is. After all, what the analytic philosopher is presumably trying to do is to shake us out of our intellectual lethargy by drawing attention to extreme and unusual cases.

Black discusses Zeno's paradoxes. In ' Achilles and the Tortoise ', he tries to show that the popular mathematical refutation of Zeno's paradoxes is inadequate, since it assumes that Achilles can perform an infinite series of discontinuous acts—a notion which he believes self-contradictory. According to Black, this paradox is created by the mathematics we use to describe our real world ; by our failure to hold separate the finite number of real things that the runner has to accomplish and the infinite series of numbers by which we describe his behaviour. In the essay ' Is Achilles Still Running ? ', Black answers objections to this analysis. He originally thought that the expression an infinite number of acts performed in a finite period of time committed their user to sinning against the ' grammar ' of commonsense language. He now thinks he may have been mistaken, and falls back on the defence that commonsense language does not permit of reference to the *indefinitely* small. The ' infinite divisibility of space ', he tells us, is a convention introduced into the language of geometry. All this is in keeping with Black's main thesis that philosophical problems largely arise from the attempt to strain ordinary language beyond its proper limits. Whether it clarifies matters to say that we cannot express the *indefinitely* small in ordinary language is doubtful. It seems to be a pale reflection of the doctrine that our percepts have a minimum extent. Further, we may ask, is the introduction of the concept of the ' indefinite divisibility of space ' simply a convention ? If, at least, some vague analogue did not exist in our ordinary thought and language, could we have arrived at the more precise notions of our technical language ?

In ' " Pragmatic " Justifications of Induction ', Black points out that the attempt to make induction respectable by treating it as a species of imperfect deduction has invariably failed. The most recent approach is to offer pragmatic considerations for following inductive policies. Our theories are assumed to be self-corrective, so that in the long run their predictions become more and more accurate. But Black argues there are quite a number of other methods we could use to forecast the future, some quite opposed to each other—soothsaying might be just as successful as orthodox induction. Indeed, we could make use of what he calls a counter-inductionist policy, namely make choices exactly opposite to that of the ' practicalist ' (the name he gives to holders of this pragmatic approach) which, he tells us, might be just as self-corrective and successful. But if someone employed a strategy opposed to our own and we followed the ' inductionist ' policy, he might occasionally be right and we wrong. Ruling out ' rigged ' situations such as biased roulette wheels, if our policy were self-corrective we would, unlike him, in the long run reach our goal. What Black's argument does bring out is that if the natural order of things were highly ' irregular ', the inductive policy would not always be the best or most successful one. Induction is only the best policy when the universe is a favourable or predictable one, and this seems to be implicit in the ' practicalist ' position. One may wonder, however, as opposed to Black, whether the very notion of a policy itself does not presuppose ' orthodox ' induction.

Philosophers, Black says, when looking for a justification for induction attempt to frame the condition in terms of a supreme principle of induction, so that it becomes logically impossible for anyone to conform. Despite the philosophical belief that the attempt to justify induction is circular, cases occur in practice in which an inductive rule has a high degree of reliability. For Black then, instead of searching for a universal principle we ought to concentrate on particular cases of inference. He points out that from early childhood onwards we have learnt to employ inductive arguments fairly successfully. However, in comparing the ordinary man's use of inductive arguments with the philosopher's attempt to justify induction, one must not overlook that the latter is largely concerned with the logic of our inductive inferences. He may be engaged in a self-defeating enquiry, but it is a different sort of one.

In the essay ' How Difficult Might Induction Be ? ', Black conjures up a demon who wants to make the future unlike the past, and who tries to define a series so irregular and complex as to make probability judgments about future events impossible. Nevertheless, I do not think that, faced with such a situation, we would be quite as helpless as Black supposes. If the demon's strategy that the past will be unlike the future were a consistent one, we might become aware of it and act accordingly. The best he could then do to outwit us would be to make the future sometimes like the past and sometimes not—in other words, employ a random strategy. If we did the same the result would be a stalemate.

In the last section of the work which is concerned with logical problems, Black tries to understand Frege's assertion that functions are incomplete. He rejects as absurd that the function itself might be considered as an incomplete object, or the function sign an incomplete proper name. This muddle apparently arises for Frege, since he still wants to think of the functional expressions as names of functions that ' stand for ' or ' refer to something ', presumably a concept. As against this Black argues that we have uses for expressions like ' the function log x ', ' To use the sign " log " correctly, we must of course know its " logical grammar ", that is to say, the rules governing its correct use ' (p. 242). But we may ask what is the criterion of a ' correct use ', that used by the best mathematicians and logicians ? And how do we decide they are the best people ? Reference to correct usage in this sort of context often seems to refer to the individual practices of approved philosophers. But why should we model ourselves, say, on the later Wittgenstein rather than Frege ?

Black's essay ' Carnap on Semantics and Logic ' is largely a critique of the use of mathematical methods to solve philosophical problems. Since it may be warmly commended to anyone who has had to struggle with Carnap's arguments, I give some account of it. Carnap, Black points out, conceives the task of philosophy as the establishment of certain relationships between sentences ; one of the most fundamental is that of ' logical consequence ' upon which deductive inference is based. This relation is for him non-factual and objective. For this reason Carnap has to define the consequence relation without assuming the meaning of such notions as ' follow from ' or ' deducible from ' as they occur in ordinary reasoning. This he does by laying down a set of transformation rules in an abstract system, which determines under what conditions one sentence is a consequence of another. These rules have a purely formal character—they merely deal with the kinds and order of the symbols concerned.

For Carnap then, to say that a relation is ' formal ' is merely to say that it holds between typographical elements. But such a view gives rise to considerable difficulties, as this will include many relations which no one would usually think of classing as logic. For example, the sentence 'James is a psychologist' has on this interpretation a certain formal relation to the sentence 'Wundt is a psychologist', namely both sentences have the same number of words, which is certainly not a logical truth. There are also cases of inference which are perfectly valid though the '$\supset$' relation or its 'if then' counterpart does not appear. Take the case of two atomic propositions such as 'This is blue'; 'This is coloured'. One can certainly infer one from the other, despite their being no bond of implication between them. Carnap could, of course, get round this difficulty by introducing an abstract semantic system and postulating as a rule of the system that 'this is coloured' can be taken as a consequence of 'This is blue'. Unfortunately, this same method of listing postulates can be extended to make any statement whatever, empirical as well as logical, a formal consequence of any other. Indeed, by postulating appropriate transformation rules it would be possible to make the categorical imperative a formal consequence of the third law of thermodynamics! We are thus unable to differentiate logical laws from empirical laws on the basis of typographical relations alone. The equating of logical with formal, and formal with typographical gives rise to paradoxes.

At times, Black's arguments require a certain amount of disentangling, as they are often more complex than they need necessarily be. I suspect this arises from the need to give particular verbal examples and to distinguish fine shades of meaning, which are such important weapons in the armoury of the philosophical lexicographer. However, unlike many practitioners of this art, Black has an excellent appreciation of mathematical and scientific modes of thought, and a fund of good sense. His work is well worth studying not only for the light it throws on central philosophical problems, but also on sometimes out of the way corners of thought.

W. Mays

Christian Theology and Natural Science: Some Questions on their Relations.
By E. L. Mascall.
Longmans, London, 1956. Pp. xxi + 328. 25s.

The Bampton Lectures, which are delivered annually at St Mary's, Oxford, do not as a rule come within the purview of this *Journal*, but those for 1956 were exceptional in that they were devoted to questions of natural science and their bearing on Christian theology and more especially because particular attention was directed to the nature of scientific theories. Dr Mascall, who is tutor in theology at Christ Church and one of the most distinguished Anglican theologians of the day, is also a former Wrangler at

Cambridge, where he made a special study of relativity and quantum theory under the direction of the late George Birtwistle. He has continued to read widely in the physical and in the biological sciences. Moreover, it is clear from the quotations and references in these lectures that he has been an assiduous student of this *Journal*. To quote his own words, his chief purpose in these lectures ' has not been so much to answer specific questions as to show that there is a large domain of thought in which it is possible for theologians and scientists to engage in intelligent, good-humoured and fruitful conversation '. This he regards as a more useful objective than the improvisation of knock-down answers to awkward questions. The book is indeed a faithful reflection of the author's erudition and urbanity.

In the first chapter, Dr Mascall discusses the contacts and conflicts which have arisen between science and theology, and lays stress on the overweening confidence which so many nineteenth-century scientists had in the finality of scientific theories. On the other hand, he condemns equally both the ' liberal ' theologians who came to terms too readily with the scientific thought of their day—much of which rapidly became obsolete—and also the ' conservative ' theologians who automatically rejected and denounced the latest scientific discoveries and theories—some of which have since come to be almost universally accepted as permanent contributions to knowledge.

Whether the author is justified in claiming that the scientific outlook of today (notably in relativity and in quantum physics) is more congenial to Christian dogma than was the scientific outlook in the last century is perhaps debatable, but he is certainly on firm ground when in his second chapter, ' The Nature of Scientific Theories ', he makes the point (p. 76) that ' Far more important than any revolution in scientific theories themselves is the revolution in the view of what a scientific theory is '.

As I see it, the vital problem at issue in the conflict between science and theology is not between two different pictures of the world but between two different ways of looking at the world. In other words, it is not between the results of scientific thought and those of theological thought but between scientific method and theological method : are they in black-and-white opposition or are they not ?

The hard core of all theology is dogma, whereas the hallmark of scientific investigation is that it is ' self-correcting '. Nevertheless, theological thought—at least as pursued by men like the author (' Neither the science nor the theology of any epoch can lay claim to absolute finality ', p. 46)—is not entirely impervious to ' correction '. On the other hand, scientific investigation requires something more than perfect open-mindedness to make it ' go '. The driving force in science is questioning, but to be fruitful it must be based on a ' hunch ' which may have to be held for a time *despite* the evidence. The great discovery often comes to the man who has the mental stamina to hold on to a theory while his critics bombard him with factual evidence against it. It is, therefore, not surprising that in practice great scientists often tend to be dogmatic. Moreover, the great scientific discovery of one age often becomes the dogma of a succeeding age, until someone reviews the evidence and finds it unsatisfactory.

Where then lies the essential difference between science and theology? It is often claimed that science, being based on things ' seen ', is demonstrable, whereas theology concerns things ' unseen ' and so is not. This is a highly simplified mode of contrast and, I believe, a distorted one. There is, however, a much less contentious way of differentiating between the two disciplines : there is one main stream of science, whereas there are many religions. However, in view of the fact that it is now generally recognised that convention plays a significant rôle in science, the surprising feature in this mode of contrast is not the multiplicity of religions, each claiming to be true, but the unique development of modern science. Indeed, it is remarkable that modern science (which has often been described, I believe wrongly, as ' organised commonsense ') ever came to be developed at all. The Hellenistic, the Byzantine, Arab, Chinese, and Maya civilisations—to name only some—all made notable contributions to mathematics and technology, but if the Black Death had completely wiped out the population of Western Europe in the middle of the fourteenth century, as it very nearly did, then, as far as we can tell, there would have been no modern science.

Although Dr Mascall's conclusion, at the end of his second chapter, that the relation today between scientific theories and the physical world is very much looser than it was and his emphasis on ' the large measure of arbitrariness as to which theory we adopt in any particular case ' lead him to the view that the modern theologian ' has no need to feel intimidated by the scientists ', the fact still remains that the strength of science, in particular vis-à-vis theology, lies in that unity which is peculiarly its own ; but how it comes to have that unity is by no means obvious and presents a fascinating general problem which will long be studied.

Space does not permit further comment on the many particular problems which Dr Mascall discusses. Two absorbing chapters on Cosmology and Contingency and on Creation in Theology and Science, are followed by one on Modern Physics and Indeterminacy, in which incidentally a lucid summary is given of Professor Popper's paper in the first volume of this *Journal*. The last three chapters of the book are on The Body and the Soul, Man's Origin and Ancestry, and The Purpose of Creation. All bear witness to the author's mastery of his material and powers of reflection. Whether or not one sympathises with his theology, one will find in his book much fascinating reading and ample evidence of learning lightly borne.

G. J. Whitrow

The Language of Taxonomy. By John R. Gregg.
Columbia University Press, New York, 1954. Pp. ix + 70. $2.50.

THE subtitle of this book accurately describes it. It is 'an application of Symbolic Logic to the study of classificatory systems'. The first three chapters give a clear account of some aspects of the theory of sets, the theory of ordered couples, and finally the theory of hierarchies. The author follows Woodger in using the term 'hierarchy' to denote a certain set of relations having special properties. Two criticisms can be made of the mode of presentation. Firstly, no distinction is made in the print used to show differences in the logical type of the variables. Individual variables have a similar notation to set variables, which are of a higher type. To those used to *Principia Mathematica* notation this produces frowns when reading some of the familiar theorems. Secondly, although it is a sign of progress in symbolic logic that notations improve, when so few authors publish in the field of applied logic it makes it unnecessarily difficult for the sympathetic reader to have to learn two notations when his interest is in the subject matter and not in the designatory merits of either system.

However, this discussion enables one to understand the subsequent account of 'SMP'—a hierarchical relation existing between Orders, Suborders, Infraorders, etc. This is shown to be necessary for a description of some of the logical relationships between the taxonomic categories in a taxonomic system.

In the remainder of the book the author gives postulates and theorems involving two further symbols 'G' and 'C' which designate respectively the set of all taxonomic categories. 'G' refers to sets of organisms and 'C' refers to sets of sets of organisms. Using these two primitive terms Gregg defines what he means by a taxonomic classificatory system and gives examples of the way his definitions and postulates can be used among mammalian systems.

This is not the place to make detailed criticisms of any of the theorems or postulates. Indeed the only way in which a criticism could possibly be made is to examine the ways in which the postulates are used in relation to what is required of the theory under construction. This book offers only the preliminaries of such a theory, and because of this some readers might be left wondering what is the point of the book. This seems to the reviewer a pity, for the book could easily merit unfair criticism because of lack of understanding of the total aim of such an approach as is used.

Because some taxonomists imagine that they can define their taxonomic categories it does not mean that they have any clear system of which their definitions are a part. They are content with rules of thumb for putting labels on to organisms. Professor Gregg is trying to do more than this. He is trying to create a system of statements which forms a calculus from which deductions could easily be made. His definitions must have a rigour which is unnecessary in mere labelling. Most taxonomists are unaware that there may be a need for theorising of this sort.

A suggestion of a possible line of thought, both for the author of the book and for any critics of this type of approach might be the following. Let us assume that taxonomy reflects the evolutionary changes which have occurred in the biological world. If this were so (and it is dogmatically asserted to be the case by most taxonomists) then the relations shown between taxonomic categories should be isomorphic in some sense with evolutionary changes which have separated the various organisms into groups. A study such as Professor Gregg has embarked on is perhaps the only way in which such an isomorphy can be shown and sensibly discussed. We look forward to seeing further progress being made along these lines.

R. F. J. Withers

Plato's Philosophy of Mathematics. By Anders Wedberg.
Almqvist & Wiksell, Sweden, 1955. Pp. 154. Sw. Crs. 19.

The first critical exposition of Plato's philosophy of mathematics is due to Aristotle, who in his presentation contained in the last two books of the *Metaphysics* goes considerably beyond the scattered and unsystematic views to be found in Plato's writings. The author of the present monograph accepts Aristotle's account as reliable. He tries to harmonise it with Plato's teaching as formulated in the dialogues and by so doing he is able to give a consistent outline of a philosophy of mathematics which, it would appear, can be quite safely attributed to the founder of the Academy.

The main task the author has set himself is to discuss and clarify the controversial problem of Plato's mathematical 'ontology'. The various problems connected with Plato's methodology of mathematics are touched upon in an incidental manner only.

As is well known Plato distinguishes between the realm of changeable and perishable sensible particulars and the realm of eternal intelligible being. The latter contains the Ideas, some of which are Mathematical Ideas. The question arises whether for Plato the subject-matter of pure mathematics consists of the Mathematical Ideas only or whether there are other entities, besides the Ideas, which form part of the domain studied by mathematics. According to Aristotle, Plato maintains the existence of two kinds of number, the Ideal Numbers, which are Ideas, and the Mathematical Numbers. The latter also belong to the realm of eternal intelligible being and are perfect instances of the former. To this distinction there corresponds an analogous distinction between the Geometrical Ideas and the Ideal Geometrical Figures, which again belong to the realm of eternal being. The Mathematical Numbers and the Ideal Geometrical Figures form the class of what Aristotle calls *intermediate objects* or *objects of mathematics*. Some Platonic scholars reject Aristotle's exposition of Plato's mathematical ontology in favour of a simpler scheme which does away with *objects of mathematics* and regards mathematics as the study of Mathematical Ideas. As has already been mentioned, the present author tries to show that Aristotle's interpretation of Plato's views is correct. Admittedly there are discrepancies but their significance should not be exaggerated. It is quite possible that Aristotle's account of Plato's mathematical ontology refers to a late period in Plato's teaching and contains views which had never been prepared for publication by Plato himself.

The book under review consists of five chapters, four appendices, and notes. Chapter i states the main problem to be discussed and explains the method which the author proposes to follow in his enquiry. Chapter ii gives a brief review of Greek mathematics in Plato's time. Chapter iii contains a lucid exposition of the theory of Ideas, which forms the background for Plato's philosophy of mathematics. Chapters iv and v deal with Plato's philosophy of geometry and arithmetic respectively. In the appendices the discussion of the last two chapters is amplified and supported by further textual evidence. It is to be regretted that there is no index.

As a whole the book distinguishes itself by brevity and remarkable clarity. Although it has been written from the philosophical point of view, it will appeal not only to philosophers but also to classical scholars and to mathematicians with philosophical leanings. It is the discreet way in which the author relates the views of Plato to some issues in the modern philosophy of mathematics that makes the book so attractive.

Czesław Lejewski

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

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A. J. Ayer, *The Problem of Knowledge*, Penguin Books, Harmondsworth, 1956, pp. 224, 3s. 6d.

J. G. Bennett, *The Dramatic Universe*, Vol. 1, Hodder & Stoughton, London, 1956, pp. xlii + 534, £2 2s.

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